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THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

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No. CCII.

CONTENTS.

PART I.—Original Communications.

Art. I. On Rabies and the Hydrophobia. By T. Lindley Kemp, M.D., ... Page 1
II. On Spurious Pregnancy and Hysteria. By Alexander Keiller, M.D., F.R.C.P.E., Physician to the Royal Infirmary, Royal Maternity Hospital, &c., Lecturer on Midwifery and the Diseases of Women and Children in the Medical School, Surgeons' Hall, Edinburgh, ... 19
III. Cases of Aneurism. By James Miller, Professor of Surgery in the University of Edinburgh, &c., &c., . . . 33
IV. On the Statics of Pregnancy. By Matthews Duncan, M.D., F.R.C.P.E., Lecturer on Midwifery, &c. &c., . . 45
V. Medical Notes on the Climate of Burmah, and on the Fevers and other Diseases which have there prevailed among European Troops. By Charles Murchison, M.D., Acting Physician to the Westminster General Dispensary, and late of the Bengal Medical Service, ... 62
VI. On the Facts and Opinions recently recorded in regard to Asiatic Cholera. By J. Warburton Begbie, M.D., Fellow of the Royal College of Physicians, lately Physician to the Cholera Hospital, Surgeon Square, Edinburgh, ... 86

PART II.—Critical Analysis.

Art. I. Lectures on the Diseases of Infancy and Childhood. By Charles West, M.D. Third Edition. London: 1854, . . . 108
II. Psychological Inquiries, in a series of Essays intended to illustrate the mutual relation of the Physical Organization and the Mental Faculties, ... 115
III. The Elements of Materia Medica and Therapeutics. By J. Pereira, M.D., ... 132
IV. Introductory Text-Book of Geology. By David Page, F.G.S., 136
V. The Cholera in Berwick Street. By the Senior Curate of St Luke's, Berwick Street, ... 137

PART III.—Progress of Medical Science.

Report of the Proceedings of the Medico-Chirurgical Society of Edinburgh during the 33d Session, 1853-54, ... 141
On Retinitis from Undue Lactation. By William Mackenzie, M.D., 146

Anomalous Uterine Pregnancies. By Professor CHIARI, . . 148
Prolapsus of the Uterus cured by Retroflexion. By Dr BERNARD SEYFERT, 151
Report upon the Syphilitic Cases treated at the large Hospital of Vienna, during the years 1852 and 1853. By Professor C. SIGMUND, . 151
Caries of Elbow-Joint, ... 154
Condition of a Joint after Resection, ... 154
Second Case of Excision of the Knee-Joint successfully treated by WM. KEITH, M.D., Senior Surgeon to the Aberdeen Royal Infirmary, . 155
Mr BUTCHER on Wounds of Arteries, ... 156
Wound of the Profunda Artery; Ligature of the Femoral below the margin of Poupart's Ligament, by a transverse incision; arrest of the bleeding, ... 156
Wound of the Posterior Tibial Artery successfully treated by pressure at the site of injury, and compression of the Popliteal and Femoral Arteries; Complication; Traumatic Delirium; Recovery, . . 157
Wound of the Ulnar Artery above the Wrist successfully treated by Compression at the Wound, and Pressure over the Brachial Artery, . 157
Extensive Suicidal Wound of the Throat; Ligature of the right and left Thyroid Arteries; Recovery, ... 158
Extensively Comminuted Fracture of both Bones of the Fore-Arm, with Extensive Hemorrhage into the Limb from Laceration of the Posterior Interosseal Artery; Recovery, ... 158
Excision of the Index Finger, together with the Metacarpal Bone, performed under the influence of Chloroform; Profuse Hemorrhage on reaction being established; Arrest of the Bleeding by powerful flexure of the injured Limb, together with gentle pressure over the Radial and Ulnar Arteries; Recovery, ... 158
Paralysis of the Portio Dura in connection with Tertiary Syphilis, . 159
Injection of Iodine and of Lactate of Iron into the Tissues as an Antidote to the Snake-poison, ... 159
Antiseptic Properties of Iodine, ... 160
Trial of Francis Forbes accused of the Murder of Ann Harvey, . 160

CASE-BOOK OF THE EDINBURGH MEDICAL AND SURGICAL JOURNAL.

I.—Cases Illustrating the Effects of the Local Application of Veratria in Tic. By JOHN CHISHOLM, M.D., Ludgate-Lodge, Ratho, . . 2
II.—Case of Anomalous Acute Disease of the Skin, resulting in exfoliation of the Cuticle. By JOHN CHISHOLM, M.D., Ratho, . . 4
III.—Spontaneous Gangrene in a Child Eight Months old. By CHARLES SIDEY, Esq., Surgeon, Edinburgh, ... 7
IV.—Case of Cancerous Disease, successfully treated by Escharotics. By JAMES D. GILLESPIE, M.D., F.R.C.S., Edinburgh, Assistant-Surgeon to the Royal Infirmary, ... 8
V.—Case of Apoplexy during Labour, fatal without convulsions. Extensive extravasation into the Brain. By W. WILLIAMSON, M.D., Physician to the Royal Infirmary, Aberdeen, &c., ... 11
VI.—Two Cases of Abdominal Aneurism. By W. T. GAIRDNER, M.D., Physician to the Royal Infirmary of Edinburgh, ... 12
VII.—Case of Labour complicated with Hydrocephalus of the Child. By MATTHEWS DUNCAN, M.D., Lecturer on Midwifery, &c. &c., . 20
VIII.—Case of Amputation of the Foot without removal of the Astragalus. By JOHN TRAILL, Esq., Surgeon to the Arbroath Infirmary, and Member of the Medico-Chirurgical Society of Edinburgh, . . 21
IX.—Case of Croup following Measles; tracheotomy on the 9th day; obliteration of the orifice of the Glottis above the canula; death on the 46th day after the operation. By Dr C. WEBER of Darmstadt, . 23

PUBLICATIONS RECEIVED.

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THE EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

No. CCIII.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. Rise and Progress of Yellow Fever in Peru. By ARCHIBALD SMITH, M.D., ... Page 165

II. Medical Notes on the Climate of Burmah, and on the Diseases which have there prevailed among European Troops. By CHARLES MURCHISON, M.D., Physician to the Westminster General Dispensary, and late of the Bengal Medical Service, 205

III. Short Account of the Cases treated in the Cholera Hospital, Surgeon Square, during the late Epidemic. By J. WARBURTON BEGBIE, M.D., Fellow of the Royal College of Physicians, Edinburgh, ... 250

IV. New Method of Treating Neuralgia by the direct application of Opiates to the painful Points. By ALEXANDER WOOD, M.D., F.R.C.P., Lecturer on Practice of Medicine, . . 265

V. Brief Memorial of the Life and Writings of the late Richard James Mackenzie, M.D., F.R.C.S., . . . 281

PART II.—CRITICAL ANALYSIS.

ART. I.—1. A Practical Treatise on the Diseases of the Eye. By WILLIAM M'KENZIE, M.D., Surgeon Oculist in Scotland in Ordinary to Her Majesty, &c., &c., ... 317

2. Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.S., F.R.C.S., &c. &c., ... 317

3. Iconographie Ophthalmologique, ou Description, avec Figures Coloriées des Maladies de l'Organ de la Vue. Par J. SICHEL, Docteur en Médecine, &c. &c., ... 317

Art. II. The Pathology of Drunkenness. By **Charles Wilson**, M.D., 329

III. The use of the Blowpipe in the Qualitative and Quantitative Examination of Minerals, Ores, Furnace Products, and other Metallic Combinations. By Professor **Plattner**, and Dr **Sheridan Muspratt**, Founder and Principal of the Royal College of Chemistry, ... 339

PART III.—Progress of Medical Science.

Trial of George Lillie Smith, M.D., and Robert Campbell, before the High Court of Justiciary, for Wilful Fire-Raising, 15th January 1855, 341

Case-Book of the Edinburgh Medical and Surgical Journal.

X. Case of Sloughing of the Labia Pudendi and Vagina, followed by partial obliteration and stricture of the Vagina, ... 25

XI. Case of Presentation of the Bladder in Labour, ... 29

XII. Lupus Exedens of the Face, fatal by Disease of the Larynx, . 31

XIII. Case of Congenital Goitre, ... 31

XIV. Case of Anæmic Palpitation.—Enlargement of the Thyroid Gland and Eyeballs, ... 33

XV. Case of Glandular Swelling accompanied by an Exanthematous Eruption, and fatal in the third week, ... 36

XVI. Case of Induction of Premature Labour for Acute Chest Complication, ... 39

PUBLICATIONS RECEIVED.

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1st JANUARY 1855.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*On Rabies and the Hydrophobia.* By T. Lindley Kemp, M.D.

Hydrophobiam ex morsu animalis rabiosi nunquam vidi.—Heberden.

The history of the opinions that have prevailed regarding the disease called Hydrophobia occurring in dogs, other domesticated animals, and the human species, is, if not a flattering, a curious illustration of the manner in which diseases are sometimes described and the nature of their pathology decided upon. The dog was probably the first animal domesticated by man, and from a very early period he has been noticed (in temperate climates at least[1]) to be liable to a disease anciently called lyssa. Of this malady delirium is a symptom, and one very common action of a delirious dog is to snap, sometimes at himself and sometimes at other objects. And as others of the domesticated animals were noticed to be liable to become affected with diseases in which delirium was a symptom, at the same time that the delirious disease was prevailing amongst dogs, it was inferred that the affection was communicated to the other animals by the dog, and through the medium of a bite.

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[1] It would almost appear that rabies in the dog, like typhus in man, is incompatible with a very hot climate.

All this was believed by the ancient Greeks, but they held that dogs thus affected had no particular disposition to bite the human species, and that if such an occurrence chanced, the bite was innocuous. " Dogs," wrote Aristotle, " are subject to three disorders,—the lyssa, the angina, and the podagra. The first of these maladies makes them mad, and all the animals that they bite become similarly affected, *man excepted.* This disease kills the dogs themselves, and every beast that is bit by a rabid animal, *man excepted.*" The remark of Xenophon, in his account of the retreat of the Ten Thousand, seems likewise to imply that this madness was a disease peculiar to dogs. The citizens of Cerasus, he said, must have thought that some lyssa, like that of dogs, had attacked the Grecian troops.[1]

A serious and fatal disease in men, however, characterized by barking and dread of water, and caused by the bite of a dog affected with the lyssa, was never described, and never supposed to really exist until the time of Asclepiades, one of the earliest of the Grecian adventurers that, attracted by the wealth and ignorance of the Romans, sought to establish themselves in their metropolis. This sophist commenced his career as a rhetorician, but not succeeding, he determined to try if medicine would be more auspicious. He had no previous knowledge, indeed, of the science or the art, but he seems to have trusted to fortune and to innovation,—the two sheet anchors of the many charlatans that have succeeded him. He denounced all violent or active remedies ; a physician, he said, ought to cure diseases *tuto, cito, et jucunde;* he pretty much dispensed with the use of drugs and (for modern systems of regular medicine are but reproductions of older ones that have had their day and become obsolete) trusted principally to water employed in various manners. Not only did he prescribe beds of various degrees of elevation to his patients, but he had baths suspended in the air, Unlike the modern hydropathic, however, he was liberal in his use of wine, and a considerable part of his popularity among those who consulted him seems to have been attributable to this. Of course he had his jokes against the Hippocratic physicians, whose science he declared was a " meditation upon death." " Whatever the whole world affirmed," said Galen, " Asclepiades contradicted." Amongst the other novelties that he propounded was, that a dog afflicted with lyssa could by a bite communicate the disease to a man, and that the disease in men so communicated was characterized among other things by an inability to swallow fluids. From this last circumstance it was named hydrophobia. Pliny speaks of the bite of a mad dog being deadly to man, and producing hydrophobia.[1]

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[1] Anab. v. 7. 26.

For two centuries, however, little faith seems to have prevailed with regard to the existence of this malady. Perhaps this may have been owing to the distrust felt by the more regular practitioners for any innovation proposed by Asclepiades. At any rate the hydrophobia was not patronized by any one until the time of Cœlius Aurelianus, who belonged not to the Hippocratic school, but to the Methodic, such as is now represented by the homœopathists and the hydropathics. This physician maintained the existence of the disease in question, that it was caused by the bite of a rabid animal, and that its symptoms were inability to drink, desire to bite, barking, convulsions, &c. These views of Cœlius seem to have met with great opposition. One party objected the improbability of a *new* malady like it being excited in the human species, and they maintained that before the time of Asclepiades no one had ever thought that he had seen such a disease. Cœlius, however, maintained that this was owing, not to the absence of hydrophobia, but the deficient observation of preceding generations. Others affirmed that the cases that he called hydrophobia were not corporeal but mental affections. Cœlius protested, however, that they were both the one and the other; in fact, hydrophobia suited well with the existing Methodic doctrines, and Cœlius made a determined stand in its favour.

And he perfectly succeeded. From his time down to the end of the last century the prevailing opinion among physicians, as is, indeed, yet the common opinion, was, that particularly during hot weather, dogs become affected with an acute disease, that is always mortal, and that is characterized by inability to swallow fluids, and the desire to bite; that the bite of a rabid dog communicates the disease to the domesticated animals and to man; that man so affected barked and was unable to drink and desired to bite, and that his saliva introduced into a wound was capable of exciting the disease in any other man whom he might so bite. Accordingly during the Middle Ages and up to almost recent times, it was considered proper to seize people who had been bitten by a rabid dog, and who showed a tendency to these symptoms, and to prevent them propagating the disorder by smothering them. Just too as, so long as witchcraft was a capital offence, people were found who had, by their own confession, all its symptoms; so individuals who had been snapt at by a dog, really barked, refused to swallow fluids, made efforts to bite, and were suffocated.

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[1] Nat. Hist. viii. 63.

Notwithstanding, however, that the belief in hydrophobia was so strong, and the practice regarding it so decided, the opinions that were entertained regarding it by the medical men were singularly vague and unsatisfactory. Even when towards the close of the eighteenth century the [illegible] logists began to systematize al[illegible] to refinement, th[illegible]
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" *April 2d,* 1732.—He went to bed in perfect health, was awakened next morning with a pain much like the former, though neither so exquisite nor equally sharp but that he could make a shift to put off with it, and even at some intervals sleep a little till ten next morning, when all of a sudden it became so violent as to force him instantly to call out for his old cure, a plentiful blooding, on which for some minutes he felt himself entirely relieved, but soon after complained of a strange anxiety, difficult breathing, coldness of his extremities, and convulsions of the diaphragm.

" I saw him before eleven, when I found his pulse much oppressed, very irregular, and often intermitting; his extremities chilled; his breathing almost suffocated, fetching heavy sighs, and very often looking ghastly; spouting saliva frequently, and roaring out. . . . . . He was immediately blooded, and seemed, for five minutes, somewhat relieved; then, all of a sudden, his extremities became cold and rigid as of a dead body, his pulse at no rate to be felt; immediately he cried out that everything was turning round him with strange rapidity. Soon after that he scarcely saw the objects, but imagined himself in such a motion. At last saw nothing, and quite bereaved of all his senses, raved in the most extraordinary manner, often starting up, and tearing everything about him; spouting perpetually great quantities of water, ready every moment to be choked in his breathing, making no complaints of his former pain, but crying out, in the most pitiful manner, how he was abandoned by all and left among flames that were consuming him, vowing eternal vengeance on all that had so used him.

" The tone of his voice often changed; he had convulsions, frequent in his face. . . . . . He had some intervals during that extraordinary shock, and called instantly for drink; but the moment he saw it, fell into the most surprising horrors, and, as it approached him, started, looked frighted, had frequent convulsions, especially about his mouth, and peevishly put it away with his hand; and then, with an air that spoke at once fright and resentment, he would stare after the drink, and soon after impatiently called for it, and repeated the same scene.

" After the last blooding, finding himself more than ever before relieved, he called hastily for a little warm milk and water; he greedily glutted a mouthful, and that very moment with great force spouted it at a great distance, and, after it, an incredible quantity of saliva, in the same manner, with the same force; and very frequently telling that, notwithstanding all his burning thirst, to swallow it was impossible, nor could he, without a kind of horror, hear of any kind of drink."[1]

Other and still more conclusive cases have been recorded by the very best authorities, and many unrecorded ones have doubtless existed. Pinel relates a very well marked one:—" There was a young French soldier who had certainly never been bitten by any animal. This person disliked his profession, and secluded himself. His comrades attributed this to cowardice, and, by way of playing a trick upon him, entered his chamber at midnight, beating the charge upon the drum, and crying that the Austrians had crossed the Rhine. Immediately he was seized with convulsions, a sensation of burning and tightness in the throat, a great dread of liquids, and the expectoration of frothy saliva. In the morning the horror of fluids was still greater, and his respiration and circulation became irregular. The symptoms increased, and he died. His body was examined, but no morbid appearance found." Savriotte has recorded a very well marked case, in which there was every symptom of hydrophobia, which terminated fatally, and in which there had certainly been no antecedent bite. In fact, there are a great many recorded cases of decided hydrophobia, in which there has been no bite, although there has been some very powerful mental impression. Also, cases of hydrophobia have occurred and terminated fatally, in which a bite, indeed, preceded the disease, but the bite of a perfectly healthy animal. In *Hufeland's Journal* there is a case reported in which an individual died five weeks after having been bitten by a dog, with every symptom of violent hydrophobia; but the dog was perfectly healthy at the time of inflicting the injury, and remained so perfectly afterwards. And in a most melancholy case of a lady who died from hydrophobia at the beginning of the present century, in Scotland, it was generally understood that although a pet dog had become rabid, it certainly did not bite its mistress, though perhaps it scratched and alarmed her.

During the last fifty years, however, very great changes of opinion have gradually taken place with regard to both rabies and hydrophobia. The pathology of the lower animals has passed from the farmer and the whipper-in to the hands of educated and skilful veterinarians. The disease *rabies* is still recognised, but it is ascertained that the animals affected by it have no dread of water; that in most cases there is no difficulty of swallowing fluids; that increase of or any alteration of the salivary secretion is frequently unnoticeable, and that the disease has no necessary connection with hot weather. Recently the idea that dogs might take rabies without being bitten by a rabid dog began to be entertained, and doubts suggested themselves whether, whatever might be the case with the dog, other rabid animals had the power of propagating the disorder by means of a bite, or, indeed, in any other manner.

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[1] Medical Essays and Observations, vol. i., p. 283. Ultimately this patient recovered.

The sentiments entertained by the medical men with regard to hydrophobia in man have also undergone a considerable change. Human beings bitten by a rabid dog are no longer thought to bark or desire to bite, and their saliva, by almost all, is regarded as innocuous. Strangely enough, as soon as these became prevalent opinions, although people bit by rabid dogs from time to time take a disease under the influence of which they die, the barking and the propensity to bite ceased. Then the difficulty of swallowing fluids began to be thought an occasional symptom only; and in fatal cases of hydrophobia lately recorded this is not a constant or prominent symptom.

A very great preponderance, too, of the supposed cause over the effect began to be noticed in rabid dogs with regard to their power of communicating the disease. A dog affected with furious madness, as it was called, that is, a case of rabies in which the delirium was strong, ran about the country snapping at every thing and animal that came in his way, and yet most of these bites clearly produced no evil consequence. It was ascertained, for instance, that of twenty people bit by an individual mad dog, nineteen were only injured by the scratch, and experienced no further ill consequences, while, were it possible to compute accurately the number of animals and men thus snapped at, and in whom no disease was set up, it would probably amount to many hundreds. In etiology, indeed, perfect exactitude is not expected, and when it is uniformly observed that out of every twenty exposed to an obvious cause, nineteen, eighteen, or sixteen became affected by a certain disease, that disease is believed to have been excited by that cause. But when the same circumstance is appplied to two, three, or five hundred similarly situated individuals, and a disease is only set up in one single instance, that circumstance cannot be considered as an exciting cause of the disease. Moreover, young children, although exposed to the cause, were not noticed to be affected by the supposed result. These and the observations, that a disease in every respect resembling hydrophobia, and ending fatally, could be excited in the human species without any bite of a dog or other rabid animal, made more than one physician doubt if the saliva of a rabid dog had the power of exciting any disease whatever in the human constitution.[1] The late Sir Isaac Pennington, professor of medicine at Cambridge, for instance, denied the existence of any such disease.

Quite recently, too, considerable scepticism has begun to arise among the veterinary gentlemen regarding many points in the pathology of the disease called rabies in the dog and other domesticated animals. The subject has been particularly investigated by Professor Dick, and he has come to the conclusion that the saliva of a rabid dog introduced into the system of another dog has no power of inducing any disease whatever.

Still there is no doubt but that there is a disease that occurs in dogs and other domesticated animals that is of an acute nature and terminates fatally, and which, in many respects, corresponds with the described rabies. It is also certain that members of the human species, who have been bitten by rabid animals, occasionally become affected by an acute and fatal disease that in many respects resembles the malady described as hydrophobia. And it certainly is of importance that our knowledge upon these two subjects should be exact.

That rabies in the dog is not produced by heat is undoubtedly true. Indeed, some of the hottest countries, as many parts of South America, have never known the disease. The dogs of Madeira, too, who, besides heat, have to endure all sorts of privations from thirst and from famine, have never had a case of rabies among them. A very cruel experiment in this respect was tried at the Veterinary School of Alfort. Three dogs were selected, during the heat of summer, and chained in the full glare of the sun; to one salted meat alone was given; to another water alone; and to the third neither food nor drink. All three died, but none of them showed the slightest symptom of rabies. Other similar experiments have been performed upon dogs with exactly the same negative result. Moreover, the disease certainly, when it is prevalent, rages as much and as intensely when the weather is cold as when it is hot, and one statistical writer upon the subject declares that one of the two months during which the disease is least frequent is August, the very hottest month of the year.

Whatever may be the popular opinion, no writer on the subject of rabies for many years past has defended the old belief that heat produces rabies. A still more important fact is, that all the negative evidence goes to prove that in a dog or other domesticated animal, the disease is not excited or caused by a *bite* of an animal previously affected. Even Youatt, so strong an advocate for the origin of rabies in the bite of a previously rabid dog, confessed that he had often examined rabid dogs on whose bodies there was no wound or the slightest indication of any injury. But, which is still stronger evidence, the rabies has often broken out in carefully-kept kennels, where there was no possibility of its having been introduced from without by means of a bite. A great many instances of this might be brought forward. We may cite just two. When rabies was, in 1849, prevailing epizootically in Scotland, the kennels of Sir John Ogilvy and Lady Menzies became affected, and in both cases it was certain that there had been no access of strange dogs; that is to say, the disease unquestionably occurring in these kennels had originated without a bite, and therefore not by inoculation from a previously rabid dog.

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[1] Bosquillon and Gerard both denied the existence of a specific hydrophobia, and Mr Green vaccinated himself with rabid saliva to indicate *his* scepticism.

Perhaps a still more decisive argument against rabies having its origin in the bite of a previously rabid dog may be brought forward; viz., that rabies is essentially an epidemic, or rather epizootic disease—it prevails for a number of months (during which great apprehension is excited), and then nearly altogether disappears for a number of years. In the south-east of Scotland, for example, where the existence of an active veterinary school renders it easy to obtain a history of the diseases that occur among the domesticated animals, it broke out in 1835, and continued most virulently for about fourteen months; and again, in 1849, when it continued for about eighteen months. But for many years before 1835, in the interval between the epizootic of 1835–36 and 1849, and since its cessation in 1850, it has not prevailed at all, no case having been brought to the school, and only an occasional rumour being heard of supposed cases having happened in country districts. It is clear that if rabies be an epidemic disease, raging for a while and then disappearing, and remaining absent for years, that either it is not excited at all by a bite, or that it requires something else besides the bite to induce it.

In support, however, of the belief that rabies in dogs and other domesticated animals is of an infectious nature, great stress has often been laid upon the fact, that the disease has often been apparently communicated from one dog to another by inoculation of the saliva. As before stated, it is indeed universally admitted in etiology that we must not expect any event to occur uniformly antecedent to any dis-

All this was believed by the ancient Greeks, but they held that dogs thus affected had no particular disposition to bite the human species, and that if such an occurrence chanced, the bite was innocuous. "Dogs," wrote Aristotle, "are subject to three disorders,—the lyssa, the angina, and the podagra. The first of these maladies makes them mad, and all the animals that they bite become similarly affected, *man excepted.* This disease kills the dogs themselves, and every beast that is bit by a rabid animal, *man excepted.*" The remark of Xenophon, in his account of the retreat of the Ten Thousand, seems likewise to imply that this madness was a disease peculiar to dogs. The citizens of Cerasus, he said, must have thought that some lyssa, like that of dogs, had attacked the Grecian troops.[1]

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And he perfectly succeeded. From his time down to the end of the last century the prevailing opinion among physicians, as is, indeed, yet the common opinion, was, that particularly during hot weather, dogs become affected with an acute disease, that is always mortal, and that is characterized by inability to swallow fluids, and the desire to bite; that the bite of a rabid dog communicates the disease to the domesticated animals and to man; that man so affected barked and was unable to drink and desired to bite, and that his saliva introduced into a wound was capable of exciting the disease in any other man whom he might so bite. Accordingly during the Middle Ages and up to almost recent times, it was considered proper to seize people who had been bitten by a rabid dog, and who showed a tendency to these symptoms, and to prevent them propagating the disorder by smothering them. Just too as, so long as witchcraft was a capital offence, people were found who had, by their own confession, all its symptoms; so individuals who had been of the street with it, we should never pronounce that the fifty subsequently attacked with cholera were so in consequence of the inoculation; but we should decide that the same men would have taken it if they had not been inoculated, and that, in fact, the inoculation had nothing whatever to do with the disease.

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[1] Nat. Hist. viii. 63.

What is the evidence in this respect with regard to rabies in the dog? Youatt, a strong advocate of the injurious nature of rabid saliva, inoculated, during the epizootic that raged in London in 1829, dogs with the saliva of dogs undoubtedly rabid, and the "inoculation failed as often as it succeeded." That is to say, that while an epidemic of rabies was prevailing, which was attacking say one-half of all the dogs in London, it attacked also a moiety of a number that had a particular experiment tried upon them. Is it likely that it was the experiment that was the cause of the rabies? A still more striking instance of this occurred at Berlin. Dr Hertwig, of the Veterinary School in that city, went on inoculating dogs for five years—he experimented altogether upon fifty-nine. Of these, forty-five were in no ways affected; and fourteen (who would probably have taken rabies just when they did if Dr Hertwig had never been born) had rabies subsequent to the inoculation.[1]

Rabies in the dog and other domesticated animals would appear in every respect to resemble those epidemics that affect man, and depend upon temporary atmospherical causes. It breaks out suddenly, prevails extensively, and suddenly disappears, not to be seen again for years. During these intervals of absence no exposure to heat, or thirst, or inoculation with saliva of previously rabid dogs can produce it. And if dogs be exposed to such agencies when it is prevailing, it is fair to conclude, that they take the disease not in consequence of such exposure, but because the malady is at that time epidemic, or, to speak correctly, epizootic.

The etiology, indeed, of all the non-contagious epidemics is so obscure, that there has always been a great desire manifested to refer them to contagious emanations from the bodies of previously affected individuals, and not to some sudden and general cause. Thus many, quite in opposition to what has been observed of the disease, have attempted to maintain the infectious nature of cholera, and to explain in that manner the sudden invasion, extensive prevalence, and rapid disappearance of that epidemic. Even Cullen put down influenza, an epidemic disease, as a *catarrhus a contagio.* And even those who perceive that the visitations of these diseases are too sudden and wide-spread to be accounted for in such a manner, can only refer them to an unknown condition of the atmosphere, and state that these epidemics thus produced almost invariably attack the mucous membranes, and that the domesticated animals are as much exposed to such diseases as the human species. They would seem, too, never to entirely disappear from a country that they have once invaded; although, when not raging, their attacks are very far between. An epidemic of cholera, or of influenza, for instance, attacks a country, and in the course of a few days thousands are affected; and, after a time, the epidemic disappears from public notice for a number of years; but, in the interval between two epidemics, an occasional sporadic case will occur, probably in some individual so strongly predisposed to take the disease, that he is affected by causes which have no influence over other men.

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[1] The perseverance with which some of these experimenters conduct their operations is almost amusing. Dr Hertwig inoculated one terrier no less than nine times in the course of three years quite unavailingly; and an unfortunate dog at Charenton was unsuccessfully worried by thirty mad dogs from time to time.

Like all such epidemic diseases, rabies is of an acute nature, and, as was pointed out by Youatt, and has indeed been long known, it is always characterized by inflammation of the mucous membrane of the fauces, often extending to the windpipe and stomach. "In the early stages," says Youatt, "there is always inflammation of the fauces," and "the membrane covering the epiglottis is inflamed." Rabies, indeed, would appear, in its earlier or milder form, to be nearly identical with influenza among men,—an epidemic febrile disorder, attended with inflammation of the mucous membrane of the back of the mouth and adjacent parts. Subsequently, at least in the cases that, from the severity of their symptoms, attract notice, and receive the name of rabies, there is violent delirium.

Destructive to the feeble in health as the epidemic inflammation of the mucous membrane about the nose, fauces, and air-passages, called influenza, is in the human species, it rarely or never is accompanied by violent delirium. On the contrary, fatal cases of it generally terminate, owing to the depressing effect of the disease upon the whole system, and not from any extension or transference of the inflammation to the brain. Is there, however, anything in the anatomy of the domesticated animals, particularly of dogs, to make us apprehend that an inflammation of the fauces, of some days' continuance, would probably implicate the brain, and produce a disease of which delirium would be a probable symptom?

It certainly would appear that there is. In all these animals, and especially in the dog, the organs of smell are very highly developed, and the ethmoid bone is truly "cribriform," that is, sieve-like. And when we consider the extent and complexity of the nervous matter between the mucous membrane of the nose and fauces and the brain, we can easily suppose that inflammation of the former might spread to the latter, and in such a case delirium be a symptom; and that this transference should be most frequent in the dog, that domesticated animal in which the olfactory powers are most developed of all.

So probable and so reasonable does this explanation and cause of the delirium of rabies seem, that, in the absence of contradictory facts, we might assume it as the true account of the pathology of the disease. But we have observations that confirm it, and we are informed that in every fatal case of rabies examined in the Edinburgh Veterinary College, whether in dogs, horses, or cattle (no sheep were examined), the brain behind the ethmoid bone was found with every mark of severe inflammation. Two morbid appearances are common to all cases of rabies, two only, but these two invariably; inflammation of the mucous membrane near the termination of the olfactory nerves, and inflammation of the brain in that portion of the brain nearest to where these nerves leave that organ.

If this be the true account of rabies in the dog, if it be an epidemic catarrh, produced by some peculiar atmospherical influence, attacking the mucous membrane at the back of the mouth, and only accidentally inducing delirium, and ending fatally by extending to the brain, and if the saliva of the affected animal be unchanged and innoxious, there is an end to the belief that the bite of a rabid dog can produce hydrophobia, or any other specific disease, in the human species. But even setting aside all this, there is other evidence sufficient to disconnect the so-called hydrophobia and rabies, and moreover to establish the true nature of the disease that does sometimes come on in a man that has been bitten by a rabid dog.

That a special and peculiar disease in man should be induced by the bite of a rabid dog, is very improbable, owing to the fact that the bite of such an animal is seldom attended by any evil consequences. Hydrophobia is a very rare disease; rabies, during the prevalence of an epidemic, a very common one; and many persons who never suffer the slightest after inconvenience, are bitten by rabid dogs. Supposing, and the supposition is an exaggeration, that half the people so bitten run to a surgeon and have the wounded part cauterized or excised, we have the remaining half who take no such precaution. Of the number of these latter, some idea may be formed from the evidence of Mr Youatt: "I was telling," he said, "the surgeon to whom I have just referred, that I had operated on nearly *four hundred* persons, and had been invariably in the habit of using the lunar caustic, and not one had died;" his reply was, "What is your *four hundred* compared to the number I have seen since I became connected with St George's Hospital: myself and colleagues have operated on more than as many *thousands*, and to our knowledge not one has been lost.

This is the experience of one London hospital, and when the other hospitals, dispensaries, &c., are taken into account, the number of people bitten by dogs in London becomes very great. Assuming that nine-tenths of those that chanced for some years before the inquiry to be bitten were bitten by healthy dogs, although the animals were imagined to be rabid, still the number of people bitten by dogs really affected must have been very great. Admitting in the meantime that the excision or cauterization would protect those who were thus treated, the moiety who neglected this precaution must have been very considerable. And yet the hydrophobia in man during this period was scarcely known in the metropolis. All the medical men who were examined before the Committee of the House of Commons in 1830, many of whom were in extensive practice, and all of whom were zealous advocates for the disease, agree as to its extreme rarity. Dr Todd Thompson stated that the first twenty years that he was in practice he never saw a single case, but that within the last ten he had seen *three* instances. Mr Earle, in his twenty-five years' experience at St Bartholomew's (where ten or twelve bitten people would be cauterized in a week), had seen "living or dead" *nine* cases; Mr Travers, notwithstanding his connexion with St Thomas's, had witnessed only *ten* cases, and he stated that in twenty years only five cases arrived at St George and St Thomas's; Dr Babington did not count his, but said, "it has not occurred to me to meet very lately with any case of the disease, and though I should be able to go a good way back in the enumeration of the cases I have seen they are not very numerous;" Sir Benjamin Brodie began his studies in 1801, and was always intimately connected with St George's Hospital, "where a great number of persons came who had been bitten by dogs supposed to be rabid," and "where a considerable number must have been so," and yet from 1801 to 1816 "he never saw a case of canine madness (in man) nor was there any one admitted into St George's Hospital. Since that period there had been several." Theother medical witnesses seem to have seen just one case a-piece.

If these bites from rabid dogs occurred in thousands of cases, in a great many of which no application of caustic was made, and only some since so bitten had the hydrophobia, it would be difficult to connect the bite with the disease. In point of fact, however, in the vast majority of the cases bitten by dogs, admittedly rabid, and in which no hydrophobia appeared, the caustic *could* not at all give protection against the disease if really infectious. The action of caustic upon a poisoned wound is simple; when that salt is applied to an animal texture, the nitric acid combines with the water that is essential to all animal structures, and the whole part touched is thereby destroyed and disorganized, including among the rest the organs of absorption. When these organs of absorption are so destroyed, if any poison be present it cannot be absorbed and taken into the system; and, therefore, cannot produce any deleterious effect. But any poisonous fluid placed in an open wound would certainly be taken up, perhaps in three, certainly in twenty minutes; and cauterization, after that time has lapsed, can have no prophylactic effect. Very rarely, however, is the caustic applied so soon as this; and usually days, and often weeks, are allowed to elapse between the infliction of the bite and the application of the caustic. Indeed, any time within a fortnight seems to be held to be immaterial.

The case, then, is this. In one large city (and other cities are quite analogous in this respect) thousands and tens of thousands are bit by rabid dogs; the immense majority of these forget all about it, and a very trifling number, not perhaps two dozen (and some of whom had probably never been bitten at all), after an interval of months, or even, according to some, after an interval of thirty years, take a disease. Is it likely—is it credible, that this disease arises from morbific matter proceeding from the mouth of a dog suffering from delirium consequent upon epidemic catarrh?

In those rare, very rare, cases which resemble the described hydrophobia, and in which there has been a bite, both the symptoms and the fatal event may be explained in one of two manners.

There is a disease, not a very common one, but sufficiently so that almost every medical man has seen an instance of it, and of which some hundreds of cases occur every year in Great Britain, called "traumatic tetanus." It is a complaint that very generally terminates fatally—is characterized by violent convulsive spasms of the muscles, and is caused by a wound, almost always a punctured wound, of a nerve. A man, for example, has his hand lacerated by a nail; in the course of a few days, and while the effects of the injury are to be seen, this dreadful convulsive disease occurs, and, in the majority of cases, after three or four days' suffering, the man dies. If the puncture be inflicted, instead of a nail, by the tooth of a dog, why should not this same tetanus come on?

But while it is clear that the bite of a dog may induce fatal traumatic tetanus, it must be admitted that most of the reported cases of hydrophobia do not resemble tetanus. In the latter disease a man receives a wound; he thinks nothing of it, save how long it will be before it heals, and never gets apprehensive about tetanus, of which, perhaps, he knows nothing; in a little time the tetanus comes on, he suffers from violent convulsions, but his mind manifests no imbecility nor peculiarity, and he hopes to get better. But in hydrophobia the case is very different; in it the affected man is almost always, indeed always, of what is called a "mobile" nervous habit or system—a long interval elapses between the injury and the access; during which he reads books about hydrophobia—consults with his friends about the disease—gives orders as to what must be done *if* he take it—and when, after a long period of anxious dread, his nervous system gives way, and convulsions come on, although no delirium be present, there is a decided affection of the mental faculties;[1] and the patient never appears to think about getting better.

In short, while it is clear that the cause of small-pox is the reception into the body of a poison, and the cause of tetanus the physical injury of a part of the nervous system, we must refer the great majority of cases of hydrophobia to that class of diseases which includes hysteria, trance, ecstacy, &c., and which are characterized by violent disorder, generally of a convulsive nature, of the muscular system, and which are produced by moral causes.

All these diseases occur only in one class of people, the members of which are said to possess *mobility* of the nervous system—a state in which sensations and emotions are too acutely felt, and have a far stronger and more lasting impression and effect upon the body than is seen in ordinary individuals, whose muscular system is little under control, and who have the *animus, nec sponte, varius et mutabilis.* It is in such individuals that moral or mental causes excite these diseases, all which powerfully affect the nervous system so as to produce the most violent spasms. These muscular contractions thus produced are more violent in hysteria than in hydrophobia; but cases of hysteria are rarely (at least rapidly) fatal, owing to so long intervals occurring between the paroxysms—whereas, in the hydrophobia, they succeed one another so rapidly, and the state of mind is so desponding, that these, combined with the inability to sleep which seems to belong to this disease, exhaust the patient, and fatally depress the circulation of the blood.

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[1] This will be evident to any one who will go through the recorded cases. It is noticed by Dr Watson. "In general," he writes, "the patient is dreadfully irritable, and apprehensive, and suspicious; and, in most cases, there is a degree of mania or delirium mixed up with the irritability; the sufferer is very garrulous and talkative. In this respect, there is a marked difference between hydrophobia and tetanus."

One link only is wanting in the chain of proof that decidedly assigns hydrophobia a place among these convulsive diseases of the nervous system produced by moral causes, and that is, cases of well-developed hydrophobia that have been cured by the application of a moral remedy. Many very decided cases of this might be brought forward. Perhaps, however, the following, for the correctness of which we vouch, may suffice:—A gentleman of gentle disposition and somewhat refined habits and pursuits, who possessed decidedly the mobile mental constitution, met with a very severe domestic calamity, which greatly depressed his spirits and increased this mobility. He was bitten by a dog that was violent and delirious, and supposed to be rabid (it was destroyed). This wound was well cauterized. Some weeks afterwards, strong excitement and convulsive movements came on, be believed that he could not swallow fluids, and his alarm and excitement became fearful. Fortunately, he was seen by a medical man who had very strong suspicions of the "moral" nature of hydrophobia; and he requested a distinguished veterinarian of his acquaintance to interfere. This gentleman visited him, and found him in a state of extreme excitement: he made use of a pious fraud, and assured him that he could not possibly have hydrophobia, because the dog that bit him had certainly not rabies, but some other disease. The patient anxiously inquired if he was sure, and on the statement being emphatically repeated, the excitement passed away, the convulsions ceased, he fell asleep, and in a little time awoke quite well.

It would, indeed, seem that the very important subject of the involuntary action of the mind upon the body, both in apparently healthy and in decidedly diseased states has never yet received that degree of attention from either the physiologist, the pathologist, or the general philosopher, which the extent and frequency of its operation deserve. And yet, perhaps, many occurrences at present inexplicable, might, were its action better understood, be rendered more clear; and many of the events that are continually happening, and which the incredulous regard as impositions, and the credulous as miraculous, might turn out to be neither, and to be explained by it.

ART. II.—*On Spurious Pregnancy and Hysteria.* By ALEXANDER KEILLER, M.D., F.R.C.P.E., Physician to the Royal Infirmary, Royal Maternity Hospital, &c., Lecturer on Midwifery and the Diseases of Women and Children, in the Medical School, Surgeons' Hall, Edinburgh.

Among the numerous nervous disorders incident to the female, none seem to present so many interesting and peculiar phases as those arising out of the physiological relations existing between the different organs of the body.

That apparently unconnected and altogether distant parts of the system have intimate and special relations existing between them, has long been sufficiently well-known; and that extensive sympathies, as they have not inaptly been termed, intimately connect the various organs or systems of organs in the female, are points too well established to require farther confirmation.

There are, however, certain peculiarities in the *characters* of such female sympathetic relations which, from the comparative rarity of their occurrence, have not been so generally observed; and which, moreover, from the difficulty of detecting the starting point, and of tracing the various links in the chain of the perturbed action, have not by any means been satisfactorily explained.

Of the many intricacies connected with the conditions referred to, none appear to be so misty, or so difficult to unravel, as those which form the subject of this communication.

In directing the attention of others to their consideration, I have been influenced, certainly not by the hope of rendering the subject less obscure, but simply by the circumstance of my having enjoyed repeated opportunities of witnessing, if not of studying, cases in which some of the more complicated and rarer forms of hysteria and spurious pregnancy were in various degrees presented, and which it will be my principal aim, in the following paper, faithfully to report.

I am well aware that the singular phenomena, so often presented in what are usually termed *hysterical* affections, are generally considered unworthy of special record unless they present some novelty in character, or illustrate some interesting point in practice, or establish, or dis-establish, some physiological or pathological opinion; and, moreover, think that the reporting of instances devoid of such useful illustration is only adding dross to the already too bulky heap of undigested material existing on the subject.

Without desiring to add to the sufficiently superfluous mass of unsatisfactory lore already existing in regard to the Protean, and too often inexplicable symptoms, which have been classed under the indefinite term *hysteria*, I shall (even at the risk of over-dealing in tedious narration) endeavour to relate the particulars of some of the more striking cases that have come under my immediate observation.

In the first instance that occurred to me of what has since been termed *spurious pregnancy*, the usual indications of utero-gestation, and subsequently, even those which immediately precede and accompany delivery, were so fully developed as to lead to the conviction, on the part of the patient, as well as others, that parturition had not only commenced, but was actually though slowly progressing.

Apart from its peculiarities in regard to pregnancy, the singularly freakish characters which it otherwise presented during its most erratic progress, may be deemed a sufficient reason for its being here fully recorded.

I deem it, however, proper to state that the points of obstetric interest in the case now referred to, were communicated by me some time ago to the Obstetrical Society, a summary of which appeared in the published records of that Society, with the promise, however, that I would afterwards more fully report the case. On being reminded of my published promise by Dr Montgomery of Dublin, I brought the seubjct before the Medico-Chirurgical Society of this city, with the view of having, if possible, some light thrown upon th[illegible] of spurious pregnancy, which (to use the wor[illegible]tgomery to me) are "so full of curious [illegible] [illegible]ich are considered, even by those who, like [illegible]d the best opportunities of studying their phenomena, to be exceedingly puzzling and far from being easy to explain.

The notes of the case are here given as taken by me at the time of its occurrence, when practising in Dundee now a considerable time ago, which may account for the style of description which, now that our attention has been more fully directed to the subject, may with truth perhaps be considered unnecessarily detailed. Instead, however, of presenting a new edition of an old report, I prefer, on the present occasion, simply to quote the particulars of the case, almost as I find them extended in my case book.

CASE I.—On the 14th September 1838, I first visited Elizabeth Berrie, a millworker, residing in Dundee, and found her complaining of symptoms which I then imagined depended on uterine derangement.

The symptoms were those indicating a hysterical tendency of constitution, together with a tumid and tympanitic abdomen, a feeling of weight over the stomach, pain in the right side, acid eructations, with frequent vomiting of meals; bowels constipated, and absence of the catamenia.

Her history, as subsequently gleaned from her mother, is as follows:—

She is nineteen years of age, and has not enjoyed good health previous to her present illness, having suffered much during her infancy; when about five years old her belly became large and tense; this continued for a considerable time, and then gradually disappeared, the abdomen however becoming distended at an after period.

When about ten years old, she was first sent to work at a mill, where she subsequently received a severe bruise from a belt of the machinery having caught her clothes. This accident occurred when she was fifteen years old, but the injury sustained must have been slight, as she was able to resume work a few days after its occurrence. Her mother, however, states that the catamenia first appeared immediately previous to this injury, that they were then copious, but after the mill accident they quite disappeared, until about a year ago, when a scanty menstrual discharge took place, which was repeated in a slight degree three weeks afterwards, these being the only two catamenial appearances from the time of her injury up to the present illness.

She continued under my care only for a short time (a fortnight) after I first saw her, but I afterwards ascertained that her symptoms increased in severity, and that she had been prescribed for by several practitioners, who, judging from the various modes of treatment said to have been adopted, must have differed in opinion (as perhaps they had good reason to do) as to the nature of the case.

I heard no more of her, except surmises that she was in a state of pregnancy, until about nine months afterwards, when I learned from some of the neighbours, whom I was then attending, that she was "near her full time," and that her accouchement, now daily looked for, would prove the incorrectness of my former diagnosis.

In June 1839, during one of my visits in the neighbourhood of her residence, I was duly apprised of her *labour* having at last commenced, and that a certain accoucheur, (who, though devoid of a legal qualification, enjoyed at that time an extensive midwifery practice among the poorer class in the district) was in attendance. Day after day I learned that "Betty," as they called her, was still in labour, and that the doctor alluded to was almost continuously in attendance upon her, and that he considered it one of the most difficult and puzzling cases he had met with. That he looked upon the case as one of extreme difficulty and danger, may be believed from the fact, that after about a fortnight's attendance, he came to the determination of relieving the patient and himself, *by performing the Cesarian section*, as he found "the bones of the pelvis so grown together that the child could never be born in the natural way, dead or alive!"

On the 25th of June, I was waited on by Mrs B., the mother, who begged me very earnestly to pay her daughter a visit as she had been "in labour for upwards of a fortnight," having been taken ill on the 9th, when she was first seen by the individual referred to, who continued in close attendance for a week, after which she settled, *i. e.* the pains subsided till the 16th, when the symptoms of parturition reappeared, and so increased as to induce her attendant to propose as the only alternative, the immediate performance of the operation of "cutting the child out of her side."

From what I had heard of the now much spoken of *difficult labour* (for by this time the poor girl's sufferings, as evinced by her screams, had become the topic of the neighbourhood), I felt anxious to ascertain the real nature of the difficulty, and therefore consented to visit the patient, provided the bold operator would, for the time being, discontinue his attendance. This being promised, I lost no time in calling, and on entering the apartment, which presented the usual preparations for an accouchement, I found a midwife, along with an assemblage of women, who had inquisitively dropped in to learn the result. After clearing the room of the useless intruders, I commenced without any misgivings as to the parturient nature of the case to make an examination, expecting indeed to find some very unusual presentation, or deformity, or other cause of detention. On examining her, however, I was not a little astonished to find the entire absence of any *uterine* tumour, or any semblance of a fœtal presentation; the os and cervix uteri natural; the former closed, and the latter, as well as the body of the uterus, in the normal unimpregnated state. The uterus, feeling light, and easily moved on the tips of the fingers, made me at once decide that it was empty, and that *pregnancy*, was not the cause of her futile attempts at delivery. I immediately informed the bystanders of the result of my examination, of my conviction that the girl had no child within her, which opinion however was any thing but credited, they all affirming, the patient herself among the rest, that the movements of the child were distinctly felt by the hand, and even seen on looking at the greatly enlarged abdomen. I had some difficulty in persuading them of the erroneousness of their notions respecting the supposed fœtal movements, which were evidently the result of increased nervous excitement, and consequent abnormal condition of the abdominal muscular walls. My opinion, however, was fully corroborated by the want of the mammary and other special signs of pregnancy. On minutely examining the peculiarly distended abdomen, she complained bitterly of the slightest percussion or pressure, and especially complained of her left side, through which she said "the child was like to leap;" the midwife and the mother at the same time positively assuring me that its movements, during her "tedious labour," were distinctly felt on the application of the hand. Her pulse at this time was as high as 120, full and bounding, and she seemed to suffer from considerable nervous excitement. I carefully examined her *per anum*, as well as *per vaginam*, in order to ascertain if any tumour existed about the uterus or its appendages, but could discover nothing unusual. The rectum appeared preternaturally distended, and the vagina (I presume from the fortnight's obstetric efforts that had been made to reach the imaginary child) sufficiently capacious. The whole muscular apparatus of the abdomen and perinæum was extremely irritable.

Believing the case to be essentially of an hysterical nature, though in doubt as to the cause of the unusual symptoms present, especially the preternaturally protuberant abdomen, I prescribed an opiate, and ordered it to be repeated until a soothing effect was produced. Notwithstanding the free and continued use of opium, hyoscyamus, &c., the symptoms were little changed—the relief from these being only temporary. Turpentine enemata, counter irritation to the abdomen, purgatives, anti-spasmodics, diuretics, &c., were severally and assiduously tried without subduing the painful symptoms. For days her screams were heard as if still in the pangs of parturition, and some weeks elapsed before her immediate neighbours were convinced that the disturbance to which they had been so long exposed did not depend on "Betty's labour pains."

Drs Bell and Nimmo visited her along with me, and corroborated my opinion as to the absence of pregnancy; but like other medical friends who afterwards saw her, could not throw any light on the pathology of the case, which afterwards became in other respects extremely puzzling.

During July and August, the abdomen continued even to increase in size, feeling tympanitic, and very irritable and painful on pressure. The bowels were generally constipated, and the pulse always quick and easily excited when awake. She sleeps little and unsoundly, awakes suddenly and with nervous starts, which she attributes to a sense of suffocation during her short intervals of repose. The treatment generally adopted in cases of Hysteria was persevered in but with little or no benefit. The already sufficiently obscure symptoms continued, and were rendered still more complicated on the 20th of August, when the case assumed a new feature, which puzzled us all to explain, or detect the true nature of. On that day she commenced to vomit quantities of bloody mucus, which collected in the mouth and pharynx, so as to prevent her from enjoying her former snatches of sleep, for no sooner was she about to forget her apparent suffering in sleep, than she suddenly awoke with a sense of suffocation produced by the accumulation of the bloody secretion in the throat, and which she seemed convulsively and painfully constrained to expectorate. This anomalous symptom continued during her long and painful indisposition, and like the other symptoms mentioned, seemed very little influenced by the remedies prescribed.

September 15th.—Former symptoms continue. For the last fortnight the bloody fluid from the mouth has increased in quantity, being about 4 oz. daily, and in appearance not unlike menstrual discharge. When asleep, it constantly oozes from the angles of the mouth: sometimes pure, sometimes frothy. She seldom sleeps longer than about 15 minutes at a time, the bloody secretion in the mouth and pharynx preventing any more lengthened repose; pulse 120; appetite almost gone. The discharges from the bowels yesterday were very copious and highly offensive, mixed apparently with bloody matter. The menstrual discharge appeared to-day, but as usual " a mere show," she having had a similar scanty flow about four weeks ago. 22d, To-day examined her, along with a few medical friends. In placing her in the upright position, the protuberance of the abdomen appeared enormous. In the posture she assumes while attempting to stand, the head and shoulders are thrown backwards fully two feet behind the heels, which awkward-looking attitude is necessary in order to enable her to support and counteract the weight which she says is continually dragging her forwards.

On applying percussion and otherwise minutely examining the greatly distended abdomen, no fluctuation, or tumour, or enlarged organ could be detected. The left side appeared, as it has all along done, more protuberant than the right, she having constantly complained of a dragging sensation from the left side of the stomach to the left iliac region, as if (to use her own words) " something was dragging her stomach down to the left side of the bottom of her belly ;" to which region she invariably pointed when describing her painful feelings. In addition to considerable spinal irritation, evinced by excessive tenderness on pressure over the lumbar region, (especially opposite the fourth lumbar vertebra) which even when slightly pressed causes much pain and excitement, the left lower extremity is observed to be somewhat contracted on the trunk. When erect she cannot place the foot flatly on the ground, the heel being slightly raised. After this time the symptoms continued almost as before, the bloody discharge, or mucus mixed with blood, constantly flowing from the depending angles of the mouth, pulse ranging from 110 to even 160 when she is raised into the upright position, darting pains from the sacrum to the hip joints, and forwards through the distended abdomen,—all of which seemed to be aggravated than otherwise by treatment. The usual concomitant of hysteria,—retention of urine—appeared about this time, and I was often sent for to relieve her, but knowing well from past experience that catheterism in hysterical females is not only a bad thing to encourage, but troublesome to attend to, I, except on two occasions, left the bladder to perform its own duty, especially as there appeared to me to exist that singular mental or emotional state which so often gives rise to malingering, which indeed I had suspected, and, as I shall afterwards show, with good reason.

During October and November the symptoms continued.

The left leg has gradually contracted until it has become completely flexed on the thigh, the heel continuing for some time in contact with the left hip, but latterly it has become implanted in the sulcus beneath the hip, the foot being singularly bent beneath the right thigh. The pain in the left leg and side has all along been more than in the right.

The menses continued to appear as before very irregularly and scantily, sometimes every three, but oftener every four weeks, and only a mere show of a single day's continuance, the symptoms becoming much aggravated at each period. Although she eats very sparingly, and although she has continued so long in such a painful state of excitement, she seems to have lost little, if any, of her usual plumpness.

December.—Continues as before. The tympanitic distention of abdomen still great, and the general uneasiness rather on the increase than otherwise. Pulse continues quick, 120. Respiration panting when awake and suffocative when asleep, apparently in consequence of the bloody secretion collecting, causing a considerable gurgling in the throat. The fluid continues, as before, to flow at intervals from both angles of the mouth. The left leg is still contracted on the thigh, the flexed knee as well as the foot, which lie bent beneath the hip in the unnatural and constrained position already mentioned, being acutely painful.

24th.—Has passed occasional bloody stools during the last three days, complaining of great pain in bowels, the abdominal distention apparently increased, the precordial region particularly being more bulging, and more than usually painful on pressure. The right leg now feels cold and pained the same as the left did, before assuming its present contracted position,—she fears its becoming like the left—pulse down to eighty to day, and is now vomiting what little food she constrains herself to take. She retains some wine, however, and relishes it. I saw her lifted out of bed to-day, the left leg firmly retained its unnatural position when in the erect attitude. This contraction has existed now for nearly three months.

January 1840.—The symptoms remain in their former aggravated form, being in no respect abated. 13th, Pulse 120. Complaining much of the pressure of the left heel on the vulva: to-day again had her lifted out of bed, presents the same curved and helpless appearance when supported in the upright position; when placed in the sitting posture, sits on the side of chair with left foot bent up under the hip; the right leg is more contracted, the toes being pointed as those of the left foot at first were. When the limbs are in any degree forcibly extended, she complains of acute pain in the abdomen, "as if the stomach was being dragged towards the lower part of the belly." The bloody secretion still flowing as before from the mouth. 22d, Visited her to-day with Dr Bell. The swelling and other symptoms as before. She retains her former plumpness, which even seems preternaturally increased around the pelvic region. Dr Bell agreed with me that the case was one of those in which moral treatment and little medicine appeared the most suitable.

During the two succeeding months of February and March, the symptoms continued more or less violent, the discharges from the bowels were occasionally copious, pultaceous, and highly offensive, the blood-like excretion from the mouth as well as the great distension of the abdomen continuing as formerly.

Towards the latter end of March, however, the long continued symptoms began to yield, the left leg, although still contracted on the thigh when in the lying posture, could now be somewhat extended when she is raised out of bed, which I insisted on being done daily; she now takes and relishes her food, and the sanguineous discharge from the mouth has gradually disappeared.

In the beginning of April the most painful symptoms had so much abated that she expressed a wish and appeared exceedingly anxious to be taken out of doors, and on Sunday the 12th April, after a confinement of upwards of ten months, with some assistance walked out for the first time. By way of redeeming a promise which she had made to a kind neighbour who had shown her great attention during her protracted illness, she succeeded in climbing (or rather in crawling up) a long stair, which led from her own to her friend's house, but unfortunately for her yet abnormally conditioned nervous system, she was there exposed to a cause of excitement which again kindled up the flame, which had just appeared to be about to expire. In the house of her friend to whom she had promised to pay this her first visit, she encountered an individual whose diagnostic powers, and most original mode of exploration, will I doubt not be here sufficiently acknowledged.

This individual, who had taken upon himself the duties of a clergyman, and who, I presume, had been paying a pastoral visit when the still protuberant abdomen and otherwise peculiar aspect of my patient first attracted his attention, desired to be made acquainted with her past sufferings and present sensations, as he had seen cases which he, judging from her general appearance, was inclined to consider as altogether analogous. On being told the history of her complaint, he immediately concluded regarding the nature of the case in strict accordance with his first surmises, to wit, that the long hidden but now discovered cause of all the anomalous symptoms which she laboured under, and was still suffering from, was nothing more nor less than a *Viper!* and as the circumstances attending this startling announcement offer a good example of the extreme credulity which obtains among the uneducated regarding remedies as well as disease, I may briefly record the procedure adopted by this gentleman to prove the correctness of his diagnosis; a procedure which however gross, absurd, and ludicrous, was considered by the patient, as well as her friends, quite sufficient to prove to their satisfaction the existence of a viper, and that moreover of the most voracious species! The patient was requested to go home and get to bed as quickly as possible; meanwhile a small piece of pork or bacon ham was ordered to be gently roasted, this done, the reverend viper catcher essayed to convince the mother, and other female attendants, of the reptile's voracity, and this he readily accomplished by simply applying the half roasted pork chop to the mouth and nose of the much excited girl, and pointed to the movements of the abdomen as yielding the looked for proof of what the experimenter described as the viper's effort to reach the savoury morsel of pork!

This wonderful discovery was no sooner thus settled beyond the suspicion of the too credulous patient and friends than the propriety of immediately acquainting me with the circumstances attending it was determined upon. The mother lost no time in running to my house to communicate the important intelligence, and which she almost breathlessly did as follows:—"Oh! Doctor, we have found it out at last, Betty has a viper within her; we saw it and she felt it as if it would leap up to her mouth;" then anxiously enumerating the farcical circumstances already detailed, and seriously announcing to me that Mr ——— intended to call upon me to give me his opinion of the case, and to advise with me regarding treatment which he had seen successfully adopted elsewhere in several cases of exactly the same kind.

This new source of irritation and excitement was immediately followed by an aggravation of the symptoms to such a degree as to render her unable to leave her bed during the remainder of April, the symptoms being simply those of relapse, being entirely similar to those I have already described. During *May*, after again trying various remedies, suggested from time to time by authorities on the subject, and by professional friends, (for every specific got fair trial,) Elaterium, Iodine, Turpentine, (internally and externally), &c., but all with very little benefit. Finding that all the medicinal and other treatment adopted had apparently no effect whatever in altering the character of the symptoms—and, I may truly enough here add, finding that I was getting tired at fighting with a shadow, whose substance I had not sufficiently detected,—I recommended a change of air and scene as the more likely means of effecting a cure.

She was consequently removed several miles into the country, but after a short trial of very ill-conditioned quarters, she was again brought to town, no worse at all events, seeing that she managed with assistance, though with much pain and difficulty, to walk some distance to the railway station. After lying a few days at home as before, she was again removed to the house of a friend in the outskirts of the town, where she remained for about six weeks. Her general health was much improved during the early part of this change of residence, she being soon able to walk about a little every day, and on one occasion (though pained, and still as large as any woman advanced in pregnancy), walked to her mother's house without assistance.

The catheter was once required during this period, and although her appetite improved, and the vomiting of blood ceased, the distension of the abdomen continued, although not to the same degree as formerly, this slight diminution of bulk being attributed by herself, to what she described as excessive discharges of blood from the bowels during stools, and which she assured me had much relieved her. Notwithstanding the improvement which had taken place during the first week or two of her stay in the country, I found her on the 16th August (when she again returned home), still labouring under her old symptoms, some of which had been only temporarily subdued. The nervous symptoms continued, her vision appeared (as she affirmed it to be) much impaired, the pupils dilated, and eyes apparently somewhat amaurotic.

For sometime afterwards the case assumed various aspects, at one time as if to undergo a rapid improvement, but only to be followed by the recurrence of the entire group of complicated symptoms already mentioned; the vomiting of blood, together with the suffocating-like appearance she presented when asleep, and the consequent watching required having urged her return home, sooner than would otherwise have taken place.

I continued watching the progress of the case from time to time, until the 29th September, when she left Dundee for Aberdeen.

10*th February* 1841—After an absence of four months, she again returned to Dundee, in a condition sufficiently good to admit of her resuming her former employment of mill-worker.

The abdomen, however, was still tumid, and although greatly improved in general health and appearance, she feared that her former symptoms were gradually recurring.

Her menstrual function has been getting more irregular, as she has had no catamenial discharge for at least two months. Her demeanour and aspect were of a decidedly hysterical character. She continued at work after this only about a fortnight, when the old symptoms re-appeared to such a degree as to oblige her again to take to bed, when, from the helpless condition she was in, in lodgings, I had her (on the 13th March) removed to the Dundee Infirmary, where she remained for a short time, without much improvement. As it daily became evident that the symptoms were becoming more and more confirmed, I urged the propriety of her again returning to Aberdeen, before she became more entirely helpless. She was accordingly removed, and (with the exception of a short note of information from her mother soon after she reached Aberdeen), I heard nothing of her after progress, until the 8th June 1849, when she made her appearance in Edinburgh (along with her father, mother, and child, for there was now a real, child in the case), for the purpose of again consulting me respecting her symptoms, which on examination I found to be analogous to those she formerly presented, although in a minor degree.

Her general appearance was considerably changed since I last saw her in Dundee (in 1841), a period of eight years having thus elapsed, and yet the abdomen continues as formerly preternaturally large and tympanitic in external character, not unlike that of advanced pregnancy.

I was then informed by her mother of the following curious circumstances respecting her *bona fide* pregnancy, and real delivery in Aberdeen, about four years ago. After leaving Dundee the swelling of the abdomen continued, but after a time she was able to walk about and work occasionally. During her pregnancy, and after her accouchement, with the exception of the abdominal signs of enlargement, all her old and formerly almost constant painful symptoms disappeared. Her general bulk and appearance were so little changed by the addition of the uterine tumor during her pregnancy, that even up to the time of delivery, no suspicion whatever existed as to her novel condition. And if their statements regarding this part of the story be credited, the girl herself seems to have been somewhat in the dark as to the real nature of the no longer spurious movements. It would appear at least that the medical gentleman who had been attending her for her old Dundee complaint as they called it, had examined her abdomen carefully the day before her delivery, at her own special request, as she thought the movements in the belly (which she confounded and connected with the movements she experienced of old, when led to believe herself really pregnant in Dundee), must be now "gathering to a height." Be that as it may, the mother declared to me that both she and the Doctor were totally ignorant and unsuspicious of the new nature of the case.

Having often thought over the case, and often spoken of the singular phenomena which it presented, (some of which I believed to be involuntary, others voluntary or emotional), I was rather pleased than otherwise to see her in Edinburgh with some of the symptoms which had formerly characterized the case, and with the view of having her watched, and the whole case thoroughly investigated, I sent her into the Maternity Hospital. A scanty menstruation appeared after her admission. On Sunday 10th, I received a note from one of the house surgeons, intimating that she had that morning "passed about one and a half pints of inky melanotic fluid" from the bladder, and requesting me to visit her, which I did in the evening, and found her asleep. The matron assured me that she had only seen one case of the kind before, and that in a woman whose word she could not depend upon, but she did not think Betty would or could practise any deception in the matter, and insisted that it could not be so, as she had "nothing in the room to blacken it with." On entering the room, however, to enquire into the probable cause of this new symptom, I found a small bottle of ink on the mantlepiece, which seemed to me to have something to do in the matter, for on handing it to the matron, and telling her to remove it along with the *black urine* which Betty (who was now wide awake) had passed, I could detect an unmistakeable expression which justified me in concluding that the urinary phenomenon mentioned was not real. On experimenting with the said bottle of ink, and the urine of another patient, a fluid in every respect similar in appearance and colour was readily procured.

11*th June.*—Visited her to-day with Dr Simpson, who examined her while under the influence of chloroform, and as this was the first case of the kind in which the singular powers of that agent were observed in diagnosing such cases, I may here mention what took place on the occasion referred to.

Before the administration of any chloroform, the abdomen was much distended, tympanitic and tender on the slightest pressure. It was, however, dull on percussion over part of the left iliac fossa. On examination *per vaginam*, Dr Simpson at first thought that the uterus was tilted over, and bound down towards that region, thus accounting for the slight dulness there. On giving her chloroform, however, and introducing the uterine sound, the uterus was found to be quite free, and in every respect natural, and much to his astonishment, and to the amazement of all present, the abdomen became quite flat, the tympanitic protuberance of the belly entirely disappearing. The urinary bladder was found to be fully distended, it being very large. The urine was drawn off by the catheter, and found to be *quite free from melanosis*. As soon as the effect of the chloroform wore off, the abdominal parietes assumed their former condition, and the belly again became (as it had so long and so very puzzlingly been), tympanitic as a drum, and so protuberant as to defy any one to detect the existence of distended bladder, or other enlargement from mere external manipulation.

15*th June*.—A consultation was held to-day at the hospital, with the view of having the opinion of the medical officers as to the real nature of the abdominal symptoms, and with the object of again testing the effect of what has since then become a most invaluable, and indeed almost indispensable diagnostic agent in abdominal enlargements in the female, especially in those still little understood cases, which (for want of a more appropriate name, and because of the concomitant symptoms being so frequently mistaken for those of pregnancy, and even I might add those of parturition, which happened in the case, which I have at so much length now related), we now are in the habit of calling, *Spurious Pregnancy*.

Before exhibiting the chloroform on this *second* occasion, the shape and character of the abdomen were carefully noticed. The whole anterior abdominal wall was regularly protuberant in front, but concave or depressed on each side towards the hypochondriac regions, in character the distention was altogether tympanitic, it being so extensive as to prevent the distinguishing of any of the abdominal organs. So soon, however, as the again administered chloroform commenced its anæsthetic power, the protuberance, as before, gradually subsided, and the concavities in the hypochondriac regions simultaneously disappeared. The front of the spinal column could now be readily traced from above downwards, its promontory being as easily distinguished through the now flaccid walls as under ordinary normal circumstances. Nothing peculiar was otherwise observed. On being roused from her deep unconscious sleep, the abdominal and other symptoms returned, though more slowly, than on the former occasion.

After remaining a month in the Maternity Hospital, under close observation, and under various treatment, she left the institution, and returned to Dundee, little improved during her stay here.

(*To be continued.*)

ART. III.—*Cases of Aneurism.* By JAMES MILLER, Professor of Surgery in the University of Edinburgh, &c., &c.

Popliteal Aneurism ; unsuccessfully treated by Pressure.

The treatment of external aneurism by pressure is still, in some measure, *subjudice.* The question must now be settled by facts, rather than by reasoning. And all facts, bearing upon the subject, in either aspect, ought to be adduced fairly and impartially—as elements of adjudication.

Myself an advocate for the trial of pressure, in all suitable cases, in preference to having at once recourse to the knife and ligature, I present the following two cases of failure as readily, though not so gladly, as if the issue had been in all respects prosperous.

CASE I. "James Hall, shoemaker, Galashiels, æt. 42. Admitted into hospital under Professor Miller, 23d March 1854.

"*History.*—States that 4½ months ago he felt stiffness in the right knee-joint, which he attributed to rheumatism. Upon getting up in the morning he experienced numbness in the foot; and he used a lotion to endeavour to remove this, but without effect. The day before his admission into hospital, a medical man told him that it was aneurism.

"*Signs upon admission.*—Patient's general health good. A popliteal aneurism is distinctly seen and felt in the right popliteal space about the size of a hen's egg, and presenting the usual appearances externally. Professor Miller thinking it a fair case for compression, ordered the patient to bed, and next day applied Carte's Compressors; one to the femoral at its lower part, the other to the femoral at its upper part. The man is very intelligent, and desirous to give a fair trial to this mode of treatment. Full instructions as regards regulating the pressure have been given—" neither great nor constant; it being sufficient to moderate, not essential to obstruct the flow of blood." The necessity of repose, absolute and with recumbency, is enjoined, along with antiphlogistic regimen, and all other means likely to favour the desired beneficial change: The patient has also been told that the object of the two compressors is, alternately, to slacken one and screw the other sufficiently tight to moderate the arterial flow; and that thus the pressure, being as it were diffused more widely, may be more easily borne.

" *27th March.*—The compressor applied to the upper part of the femoral has caused so much pain and uneasiness that it has been discarded. Pressure is now kept up with the other one.

" *5th April.*—Complains of much pain, and there is considerable swelling and œdema of the limb. Carte's other compressor is abandoned; and a five-pound lump of lead maintained in position by an elastic band, as used in the Dublin Hospital, is found very serviceable. It controls the flow of blood as well, and causes much less pain than either of Carte's Compressors.

" *1st Aug.*—The patient has persevered now 17 weeks. He has been using the lead weight constantly; though every now and then, for a day, Carte's Compressor to the lower part of the femoral has been applied. There is certainly some amount of consolidation in the tumour. It is harder, and perhaps slightly diminished in size; but the pulsation is still *very distinct.* Flannel bandages have been kept to the limb to repress the swelling, which has been at times very great from the patient exercising too much pressure under the hope of effecting a speedier cure.

" *3d Aug.*—The patient is very desirous to have the superficial femoral tied. He is told to persevere some time longer, using the lead weight by day, and Carte's Compressor by night.

" *10th Aug.*—Professor Miller told the patient to take off all pressure, and merely to keep the limb quiet.

" On examining the tumour sometime after the pressure had been removed, it appears to be a great deal harder in the popliteal space, but still pulsates strongly and distinctly, and appears to have become more caudate (towards the hip).

"It also appears to have increased to the outside, where it now distinctly pulsates. The patient expresses relief since the pressure has been removed, of which he complained much latterly.

"22*d Aug.*—Since last report, no pressure has been applied —12 days; the limb has returned to its natural size; and the tumour is in much the same state.

"24*th Aug.*—(Five months exactly after the beginning of the treatment by pressure) Professor Miller tied the superficial femoral to-day at 1 P.M.—The parts were observed to be rather more matted together than is usually the case.

"The limb was covered with cotton-wadding, and the patient ordered to be kept on low diet. The tumour is silent. Limb of the natural temperature.

"8 P.M.—A slight thrill is perceptible in the tumour, leg quite warm and no uneasiness or pain. He has been very sick from his chloroform.

"25*th Aug.*—Thrill in the tumour is less than last night. Limb warm and comfortable. Wound looks well. Complains of a very severe pain in his back, almost preventing his breathing. It appears to be principally muscular. Sickness continued during the night. Pulse natural. Treatment by colchicum internally and chloroform locally.

"8 P. M.—Thrill much the same as at last report, perhaps rather less distinct. Limb continues in the same state. Complains a good deal of the pain of the wound. Pain in back much the same. Pulse natural.

"26*th Aug.*—Thrill scarcely perceptible, tumour feels hard and leathery. Limb warm, and no pain. Was very much troubled during the night by an exacerbation of the rheumatism.

"27*th Aug.*—Thrill quite gone from the tumour, limb continues the same as at last visit. Rheumatism quite gone.

"28*th Aug.*—Continues well. Tumour feels quite hard.

"30*th Aug.*—Continues well; but a slight thrill is perceptible in the tumour. Limb still continues warm and comfortable.

"1*st Sept.*—Thrill still continues, but very slight.

"15*th Sept*—Thrill still present, but that is all. The ligature is still in the wound, which is looking quite healthy and nearly healed. Leg has remained quite comfortable all along.

"18*th Sept.*—Ligature came away to-day, pulsation very slight indeed.

"28*th Sept.*—Pulsation on the outside of the tumour feels rather more distinct than it has done for the last few days.

"10*th Oct.*—Tumour has at length become silent, and is very much diminished in size.

"14*th Oct.*—Dismissed at his own request; with a view to amendment of the general health, by country air, and home comforts. Tumour silent.

"*P.S.*—Some days after his dismissal he says that the tumour is daily decreasing, but that there is still a slight pulsation in it."

This is the report recorded and extracted by the clerk. Under the circumstances, I have presented it in its original form. I wish a plain unvarnished tale, afforded by the observation of other eyes than my own.

In addition—anxious "nothing to extenuate, or set down aught in" partiality—and knowing my patient to be both honest and intelligent—I requested him to furnish me with his own impression of his case, after deliberate consideration of it with this view.

And the following excellent statement, in the preparation of which I believe him to have been influenced by no bias whatever, I now subjoin:—

"Royal Infirmary, Edinburgh,
October 12*th* 1854.

"To Professor Miller.

"Sir,—In noting down what I remember concerning the disease and treatment of my leg, after being admitted as a patient into the Royal Infirmary, I may say concerning the screw when it was first applied, that I thought it was quite impossible for me to suffer it to remain, the pain was so excessive and caused a peculiar burning sensation all the way down the leg. However, I was given to understand that if I wanted to be cured by that means, I must do my best to endure the pain; I, therefore, continued the screw for about a month, but not so tight as to entirely stop the pulsation in the tumour.

"At the end of about a month or five weeks my leg got so much swollen, especially at the ancle and foot, that the screw was taken away and a lead weight brought to me and belted round my thigh; but it was much too light to stop the current of blood, therefore I had always to keep pressing upon it with my hands. I did not think it was doing much good, as I could not keep up a regular pressure. I was again induced to apply the screw, when I found that the button at its bottom was too small, thereby rendering it extremely difficult to confine it to the part intended. I took it away and fastened the lead to it, as it had a much larger surface, did not cause such a pain, and less of the screw was required. Although my leg continued to be much swollen, I persevered with it for about a month or so; that would be about two months and a week altogether, from the time that the compression was first applied. About this time it was thought proper to take away the screw again, in consequence of the excessively swollen state of my leg. A second lead weight much heavier than the first was given to me and belted round my thigh same way as the first. Now from the end of the first month until about this time I thought I was getting better, as the tumour seemed to be getting harder and smaller. This was about the beginning of the fourth month. I continued with the lead weight for a short time longer, when the screw was brought to me again, and I was desired to use the weight and it alternately, the one on the thigh and the other up in the groin; I did so for some time, but by this was beginning to think that all would not do, for I felt that the disease was progressing, that my knee was getting more troublesome, and swelling more both on the outside of the knee, and up the inside of the thigh. The leg became more painful in these parts than ever it had been, and I thought it was more difficult to stop the pulsation. This was at the end of the fourth month, or the first week of the fifth.

"I think that I would have been operated upon then, but requested to be allowed to try the compression a little longer, as I was anxious to give it another determined trial, fancying that, perhaps, I might not have been sufficiently persevering in my former applications. I commenced again with the screw and weight, using them alternately as before, but with greater firmness, never removing the one until I had the other fixed, and all the while suffering great pain. I continued this for a short time, but finding that it was doing no good, and that the disease was progressing, I gave it up, and requested to be operated upon. This was at the end of the fifth month that I had been using compression, and I thought that I was better at the end of the third month than I was now when I had fairly resolved to give it up.

"It is now six weeks since I was operated upon, and I have felt but very little pain in the leg since, can use it pretty freely, feel no pain or inconvenience from the operation having been performed, firmly believe that I am getting better, and expect to be home very soon.

"The foregoing is as brief and connected a statement of the treatment I have received, since entering the Infirmary, as

I am at present able to furnish.—JOHN HALL, 7, Surgical Ward, Royal Infirmary, Edinburgh."

CASE II.—I am indebted for the report of this case to my friend Dr Johnstone, of Montrose. Having discarded the large pelvic apparatus of Carte, at an early period of Hall's treatment, I was glad to be able to forward it to Montrose, in the hope that it might assist in the reduction of the aneurism there. That hope was doomed to disappointment, however, as the subjoined statement by Dr Johnstone will explain:—

"Thomas Young, aged forty-one, was admitted into the Royal Infirmary, Montrose, 24th March 1854, under the care of Dr Johnstone, with an aneurism the size of a small orange in the left popliteal space.

The patient, who was at one period in the army, had been for the last few years employed in lifting and stowing heavy bales of flax; an occupation requiring great muscular exertion of the lower extremities. In the month of July last year, he felt a swelling in the left ham which caused so much pain and difficulty of walking, that he was obliged to relinquish the laborious work he was engaged at, and take to the easier occupation of weaving.

The aneurismal swelling has gradually increased in size, and for sometime before his admission into the Infirmary the pain has been so severe as to incapacitate him from work of any kind. The symptoms of aneurism are well marked. Pressure upon the femoral artery completely restrains the pulsation in the sac, and renders it soft and flaccid. The general health is moderately good, and no disease can be detected in the heart, or in any other part of the arterial system.

He was ordered low diet, with as small an allowance of liquids as possible, ten drops of tincture of digitalis three times a-day, and a brisk saline cathartic three times a-week.

On the 2d April, Carte's compressor was applied to the artery in the groin. At first the pressure was borne for four hours, afterwards for shorter periods, with intervals of fifteen or twenty minutes. The compression did not entirely extinguish the pulsation. On the 6th April, a padded iron hoop with screw and boxwood pad attached, was applied so as to make pressure over the superficial femoral artery alternately with Carte's instrument. As the patient could sleep none when either instrument was screwed sufficiently tight to interrupt the circulation, a heavy weight of lead was fastened by means of bandaging upon the artery in the groin, for five or six hours every night; and the other instruments removed. And although the amount of pressure produced by this measure did not control the circulation in the sac so effectually as the other instruments did, its effect was very decided.

The doses of digitalis were sometimes increased and sometimes intermitted, according to their effect on the heart. On 30th May, a compress was applied over the swelling, and a flannel bandage from the toes over the knee-joint; but on account of an excoriation forming over the sac, this had to be removed on 7th June.

These measures were persevered in steadily, until 16th August; and the patient, being an intelligent and resolute man, seconded and assisted in every way.

The following notes taken at intervals will show the progress of the case:—

10*th April.*—A sensible flattening of the tumour can now be perceived.

17*th April.*—To-day on carefully examining the tumour, its pulsation and size seem gradually diminishing.

10*th May.*—The tumour is now much flatter and less defined than formerly, and altogether smaller. The pulsation is now weaker on the removal of all pressure.

30*th May.*—Tumour still diminishing, and pulsation weaker.

7*th June.*—Bandage and compress removed; tumour feels very much flattened, and pulsation greatly weakened. The swelling is now the size of a walnut.

16*th August.*—For sometime back, little or no progress has been made. The sac is much reduced in size; but there is still a considerable amount of pulsation, although pressure has been perseveringly continued. His health and spirits are now beginning to flag; and it was resolved to-day, in consultation with my colleague, Dr Lawrence, to lay aside all pressure with a view to getting the parts into a better condition for the application of a ligature to the artery.

23*d August.*—This forenoon a ligature was applied to the femoral artery under the inner border of the sartorius; the muscle being slightly turned aside. Pulsation immediately ceased. In the evening, he complained of pain in the limb, and seemed a good deal excited from the effects of the chloroform.

24*th August.*—The limb after the operation had been enveloped in flannel and cotton wadding, and to-day, the temperature being found natural, the wrapping was laid aside. Still complains of pains in the limb, and pulse a little excited.

25th August.—The slightest degree of pulsation can be felt in the sac, which is somewhat painful.

30th August.—Doing well. No pulsation in sac since date of last report.

14th Sept.—The ligature came away to-day with the dressing, and has probably been loose for a day or two.

15th Oct.—Young was dismissed to-day, walking very well upon crutches. The tumour in the ham is gradually diminishing, and the pain in the limb is almost gone.

You will see by referring to the "Case," that there was no undue collateral circulation. Indeed, I attribute the success of the ligature, and the absence of any troublesome symptom after its application, to the effects of the pressure upon the collateral circulation. I applied the ligature as low down as possible."

The following observations naturally occur, in connexion with these two cases:—

1. *The Pressure was not easily borne.*—It was placed entirely under the patient's control, with the explanation that actual arrest of the circulation was not necessary; and notwithstanding, it proved a source of such irritation as to necessitate frequent intervals of cessation, as well as change of instruments. I am inclined to think that an effective, simple, and easy method of applying the pressure is yet to be discovered. In Hall's case, the simple lump of lead was certainly preferable to the more complicated apparatus.

2. *The Pressure requires careful watching.*—At times the patient is apt to become too energetic; and screwing down the instrument with desperate violence, even for a short time, he may excoriate the part, thereby interrupting the treatment very seriously, besides incurring the risk of erysipelas, and other inflammatory accidents on the surface. Attention must also be had to the swelling of the limb. Even moderate pressure, too uninterruptedly maintained, is apt to induce very considerable œdema of the foot and leg, with more or less numbness: and in Hall there was besides, all through the treatment, some swelling of the adductor mass of muscles in the immediate neighbourhood of the compressed part. Cessation of pressure, occasionally, seems quite sufficient for preventing troublesome or dangerous excess of such swellings. But, likewise, the tumour itself must be looked to. It may be irritated by the pressure above; growing more acute and active, as it were, at times; and when this is observed in any degree, all pressure must of course be abandoned till the aneurismal excitement has

passed away. Care must also be taken that the patient does not apply the instrument too low down. Pressure in the immediate neighbourhood of the tumour may occasion a worse result than mere excitement; the aneurism, by sudden enlargement, may become *diffuse.* And, indeed, the risk of such an accident must constantly be adverted to throughout the whole treatment; the surgeon being prepared to check any such aggravation of the tumour, provided cessation of pressure does not succeed in restoring quiet, by at once having recourse to the ligature.

In Hall's case, I did not observe any risk of inflammatory evil in the vein. At no time did any local or constitutional sign of this shew itself. Amid many interruptions from other causes, the vein was always free and unsuspected. And this circumstance has an obvious and important bearing upon the practicability of such pressure being maintained safely, even for a very considerable period.

3. In Hall, *the collateral circulation was very decidedly affected.*—During the first weeks, I had great hope of obtaining a cure. The tumour became slowly harder, smaller, and less strong in pulsation. But after the third month, the impulse became decidedly more full, and with this change the hardening and lessening of the tumour stopped, and the tendency was rather the other way. I have no hesitation in attributing this result to the full establishment of most free collateral circulation which the continued pressure had produced. And in any future case, were I to observe a similar change, I should at once supersede pressure by the ligature; satisfied that by such establishment of free collateral circulation the success of pressure alone is rendered almost impossible, while at the same time delay of the ligature were most imprudent, lest, from the same cause, even its more abrupt and decided effect on the supply of blood might find some difficulty in silencing and shutting up the tumour. This difficulty after the ligature was experienced very decidedly in Hall's case. The return-pulsation was more than a "thrill" as stated in the hospital report. It was a most decided impulse, with expansion; at the end of a week, stronger than on the day after the operation. And although its first appearance gave no alarm, its continuance and increase caused me, for a time, serious uneasiness and apprehension as to the ultimate result; all the more as we know that ligature of the femoral *has* failed to cure popliteal aneurisms, the tumours, but little checked at the time of the operation, continuing to throb and grow as before. Had it not been for the constant use of direct uniform pressure over

the tumour, with careful bandaging from the toes upwards—begun and maintained so soon as the return-pulsation became formidable—it is more than probable that the aneurism had been still in progress of growth, instead of cure. As it happened, the existence of decided pulsation in the tumour, with very imperfect consolidation and diminution of bulk, continued for six weeks after the application of the ligature, with the exception of three days between the fourth and seventh day after the operation, during which no pulsation or thrill was observed. From the end of the sixth week all was silent for about eight days; but then the thrill returned; very slight and weak, however, and not such as to interfere with the satisfactory progress of the tumour. The change on the collateral circulation, then, had a two-fold injurious effect; it rendered the further continuance of pressure abortive, and it gave trouble even after the ligature had been applied.

So far it did harm: but in another point of view it did good. From its freeness, the continuance of pressure was rendered perfectly *safe*, as regards the general circulation of the limb. But what of that, if such continued pressure were sure to prove ineffectual? True. But from the same cause, the immediate effect of the ligature, when had recourse to, was also rendered perfectly safe, in so far as its first risk was concerned. There never was the slightest threat of gangrene in either case. Indeed, in Hall's, the temperature and general comfort of the limb were scarcely disturbed at all by the operation—a feature of his case which could not fail to strike any attentive observer.

In Dr Johnstone's case, it merits special notice that the collateral circulation—though it may have contributed to, or caused the non-success of the pressure,—gave no trouble whatever after the operation. The return-pulsation was not excessive, and ceased in the usual time and way. And, as in Hall, Dr Johnstone distinctly attributes the comfortable state of the limb after the ligature, to the freeness of the collateral circulation which the pressure had established.

In future, after prolonged pressure, were I to find the tumour to cease growing less, and begin to enlarge, with other obvious indications of increased collateral circulation, I would at once abandon the treatment by pressure; *dissatisfied* with it, so far as it had failed to cure. But, taking up the knife and ligature so soon as the compressed parts had sufficiently recovered themselves, I should feel *indebted to* the previous pressure, in having so prepared the circulation of the limb as to remove all risk of gangrene from an operation performed with ordinary care and skill. And, further, under such circumstances, I should not consider myself warranted in resuming the pressure, or in any other way delaying the operation, lest by so doing I should peril the success even of the ligature.

4. *The effect on the pressed parts is important.*—In Hall there was always more or less swelling, with some tenderness, in the thigh, beneath and around the compressor. Sometimes I fancied I could trace a hard and tender femoral artery; but, as already said, I never was led to suspect the vein. I believe that such a state of matters might be permitted to continue, with due precautions, for a very considerable time; but when it comes to be spread over a period of five months or so, there is a risk of the parts implicated in the operation becoming so infiltrated and incorporated, under the influences of a minor and chronic inflammatory process, as to give trouble at least, if not risk, in exact application of the ligature. Some years ago, in tying a femoral artery, to which I had previously applied ineffectual pressure for a fortnight, I found the textures all as fresh and free as if no pressure had been used at all. But in Hall's case, it was very different. The textures were dense; and it required unusual time and caution to separate the vessel, so as to permit an accurate transit to the needle. This is, no doubt, a decided objection to ineffective pressure, much prolonged. At the same time, care and caution in the operation will suffice to elude all danger from that cause, in the mere act of performance. And I do not know that such a state of parts is likely to prove unfavourable to the satisfactory progress of the ligature afterwards. On the contrary, the presence and persistence of a minor inflammatory process in the *part*, with such a moderated and modified state of *system* as the confinement and regimen connected with long continued pressure must necessarily produce, seem favourable to the occurrence of such plastic results in the artery as we desire.

5. This leads me to remark more prominently, that after prolonged pressure *the state of system is likely to be favourable to the ultimate operation.*—Many surgeons are of opinion that patients should be more methodically prepared for capital operations than they are, when the circumstances will permit of such delay,—as in stone, quiet aneurisms, chronic disease of bone, slowly growing tumours, &c. Perhaps they are right; at all events, we hear of one American lithotomist, enjoying great celebrity, who attributes all his remarkable success to the one circumstance of careful and

prolonged constitutional preparation of his patients; weeks, and even months, being devoted to this, if need be. And surely the careful diet, the regulation of the bowels, the recumbent posture, the attention to all excretions and secretions, which are inseparable from a month or two's use of pressure, will tell favourably on the system so far as the endurance of any operation is concerned; provided always, that an opportunity for its performance is chosen, free from all intercurrent excitement or irritation, whether local or constitutional.

One precaution connected with the operation was not sufficiently attended to in the case of Hall. While the patient was on the operating table, he should have been more carefully protected against cold-taking, after so long and unbroken a confinement to bed. The consequence was a smart attack of rheumatism in the lumbar region. And if this had become developed into general rheumatism with fever, a state of system the most formidable would have been induced at the critical time of the ligature's expected separation. Fortunately, it yielded in time to colchicum. Simple precautions will avert such risk in future.

6. An odd parallelism runs through these two cases, as to date of admission, age of patient, date of operation, and general progress. They have obviously the same bearings on the question of comparative merit between pressure and ligature in the treatment of aneurism. They speak both for and against pressure; and the case may be succinctly stated thus :—*Against*. 1. The pressure was not well borne. It was generally irksome, sometimes painful, and occasionally intolerable. 2. In Hall, the effect on the collateral circulation proved troublesome in the tumour, and the effect on the compressed parts caused a little difficulty in the operation. 3. In both cases, the plan of treatment failed, though prolonged over the extensive period of about five months:—*In favour*. 1. The fact of pressure, occasionally considerable, having been so long used with impunity, is a strong testimony to the safety of the practice, with ordinary caution. 2. The establishment of free collateral circulation, before the operation by ligature, was favourable to the result of that operation so far as the safety of the limb was concerned. 3. The repose and regulation of the system, attendant on the use of pressure, gave a state of constitution obviously favourable to the operation in its general results. There was, especially, less risk of inflammatory accidents in connexion with the ligature.

In conclusion, I have to apologise to the friends and promoters of the modern treatment of aneurism by pressure, more especially to our esteemed brethren in Dublin, for the lame assistance which I have hitherto been enabled to afford to their movement. Why I should have hitherto failed in all the cases of trial, I know not. I believe the principle of the treatment is both scientific and sound, and ought to be put in practice in many cases of external aneurism, at least before having recourse to ligature, being convinced that it is likely to succeed in not a few, while, if used with caution, as to time and degree, it cannot interfere with, but will rather promote, the success of the operation subsequently. I do not think it likely that I shall ever again continue ineffectual pressure *so long* as in the case of Hall. Baffled, I shall earlier have recourse to ligature. But, at the same time, I am far from abandoning the practice; and hope yet to have the pleasure of bringing forward more than one example of success.

ART. IV.—*On the Statics of Pregnancy.* By MATTHEWS DUNCAN, M.D., F.R.C.P.E., Lecturer on Midwifery, &c. &c.

In considering the points of this subject which I propose to discuss, I shall adopt the following arrangement:—

1. The Position of the Uterus.
2. The Position of the Fœtus in Utero.
3. The Position of the Pregnant Female.

In the course of these remarks it will be observed, that as much labour is spent in clearing the field of errors and misconceptions as in establishing the truth.

Before commencing, I must guard the reader from supposing that in defining angles, &c., I wish to imply the real existence of mathematical exactness in the subject. When I use numbers in this way, it is merely to express a near, perhaps, the nearest approximation to the reality. And I resort to them in order to facilitate the application to the anatomical circumstances so expressed of mechanical principles, which can be illustrated only by mathematically exact statements.

I. THE POSITION OF THE UTERUS.

In the healthy unimpregnated female, the uterus, an organ of about two and a half inches in length, generally lies with its fundus near the centre of the imaginary plane of the brim of the pelvis, and having its long axis in a direction corresponding to the axis of the brim, that is, an imaginary line drawn at right angles to the plane of the brim from its centre. (See Fig. 2.)

The unimpregnated uterus, weighing about an ounce, is so delicately and unstably poised in its position, that the common description of it as *floating* there, is an extremely apt and happy use of that term. In accordance with this, we find that the first change produced by the increase of its weight and bulk in pregnancy, is that it sinks in the pelvis. Gradually increasing in size to dimensions greater than those of this bony ring, it rises out of that cavity, and is described as coming to rest upon its brim. But the uterus cannot without risk of misapprehension be described as resting upon the brim of the pelvis. These words imply that in pregnancy, the mass and weight of the uterus repose upon the alternately soft and muscular, or harder and bony walls of the brim in the living female.

It must be remembered, that the brim of the pelvis is in the erect, the attitude understood in all such discussions, much nearer vertical than horizontal, being inclined to the horizon at an angle, (see fig. 2), of above 60°,[1] a position in which it is evident it can give no support in the ordinary acceptation of that term. If we turn to the body to be supported, we shall find its circumstances totally inconsistent with this description. The long axis of the uterus is in all the earlier months of pregnancy inclined to the horizon at about the same angle as the axis of the brim, that is, an angle of about 30°, (see figs. 2 and 4) to the horizon. In the latter months the angle of inclination of the upper part at least, exceeds 30° somewhat. (See Fig. 6.) From its nearly oviform shape, with the small end lowest, and the nearly uniform density of its contents, it may be predicated that its centre of gravity is nearly the centre of its mass. Now every body tends to fall in a vertical line from the horizon, passing through its centre of gravity. A necessary condition for the support of any body is that its centre of gravity be supported. This necessary condition is not fulfilled to the pregnant uterus by the pelvis. Consequently, the pelvis cannot be correctly described as *the* part supporting the uterus.

The pregnant uterus is supported on every side by the soft and elastic structures which everywhere surround it, as closely as if it were enveloped in a fluid. No single part can be described as *the* part supporting it. But with this qualification, the nearest approximation to a correct statement of the conditions of its equilibrum is the following:—It reposes upon an inclined plane formed by the anterior abdominal wall, and is prevented from moving down the plane by the pelvis. To be at rest upon an inclined plane, a body must have a power exerted upon it to arrest its motion down the plane. In the pregnant female, this is the power exercised by the brim of the pelvis. The power required to resist the pressure down the plane, (that is, the pressure on the brim of the pelvis,) is to the vertical pressure on the inclined plane, (that is, the pressure upon the anterior abdominal wall,) as the height of the plane to its length. Now, in an inclined plane, which rises from the horizon at about an angle of 30°, the pressure on the plane itself, (that is, on the anterior abdominal wall) must be greater than the pressure in the direction down the plane in the proportion of 87 to 50.[1] Thus it is found that the chief support of the uterus is the extensive, equable, soft and elastic abdominal wall, not the comparatively limited, unequable, hard and inelastic pelvic brim.

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[1] The brim of the bony pelvis has an inclination of 60° to the horizon, according to the researches of Nægele, on the female. This angle is increased by the psoæ and iliaci muscles, which cover the sides of the brim, being thicker superiorly than inferiorly.

It is not necessary to point out at any length the evident application of these views in explaining the production of anterior obliquity of the pregnant uterus, or of pendulous belly. It is upon the anterior abdominal flap that the chief weight of the pregnant uterus reposes, and this part is consequently most liable to be morbidly displaced by this pressure. "There is an affection," . . (says Montgomery,)[2] "constantly mistaken for and treated as local inflammation; I speak of a pain felt at either side about the margin of the ribs, and arising from the dragging of the muscles at their insertions in that situation, especially of the oblique at their superior attachment." The same circumstances as throw the weight of the uterus on the anterior abdominal muscles, remove the main pressure of the organs from the brim of the pelvis, through which it would otherwise be more liable to prolapse, than it really is.

It is here necessary to point out that great misconceptions are entertained in regard to the position of the uterus, in different positions of the female. For instance, in attempting to disprove the gravitation-theory of the position of the fœtus in utero, to which we shall have afterwards to allude, it has been argued[1] that that doctrine presupposes that the mother's body is in the vertical position, in order that the gravitation of the fœtal head may have the effect attributed to it, and that the horizontal position assumed during repose, or from other causes, ought to afford many more chances than the statistical results shew, of the head assuming, by gravitation, other positions than the ordinary one. This argument, to have any force whatever, must imply that in the vertical position of the female, the uterus is vertical, or nearly so; and in the horizontal or recumbent posture, horizontal, or nearly so. But this is very wide of the truth; nay, it is totally opposed to it.[2] In the erect position of the female, the uterus is, in all the earlier months of pregnancy, not vertical, but inclined to the horizon at a moderate angle of about 30° (See Fig. 4.) In the latter months of pregnancy, the uterus is generally, though not always, directed at an angle somewhat nearer half a right angle (See Fig. 6), by the resistance of the anterior abdominal wall to its growing in its

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[1] For this calculation, as for other kindnesses, I am indebted to Mr J. Elliott, Lecturer on Natural Philosophy.

[2] Signs and Symptoms of Pregnancy, p. 7.

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[1] M. Dubois, who speaks of the "almost vertical direction of the uterus in the erect position," adds—"We cannot refrain from remarking, that there is a considerable number of women who, for the sake of their health, pass the greater part of their pregnancy in a position almost horizontal; and although, even in this position, the uterus may maintain something of its ordinary direction, nobody will believe that the slight inclination, which its walls offer, is sufficient to favour the descent of the head towards the uterine orifice. We are, nevertheless, not aware that, in these circumstances, the presentations of the head are less common than otherwise.—*Mem. de l'Acad. Roy. de Med.* Tom. ii. p. 270.

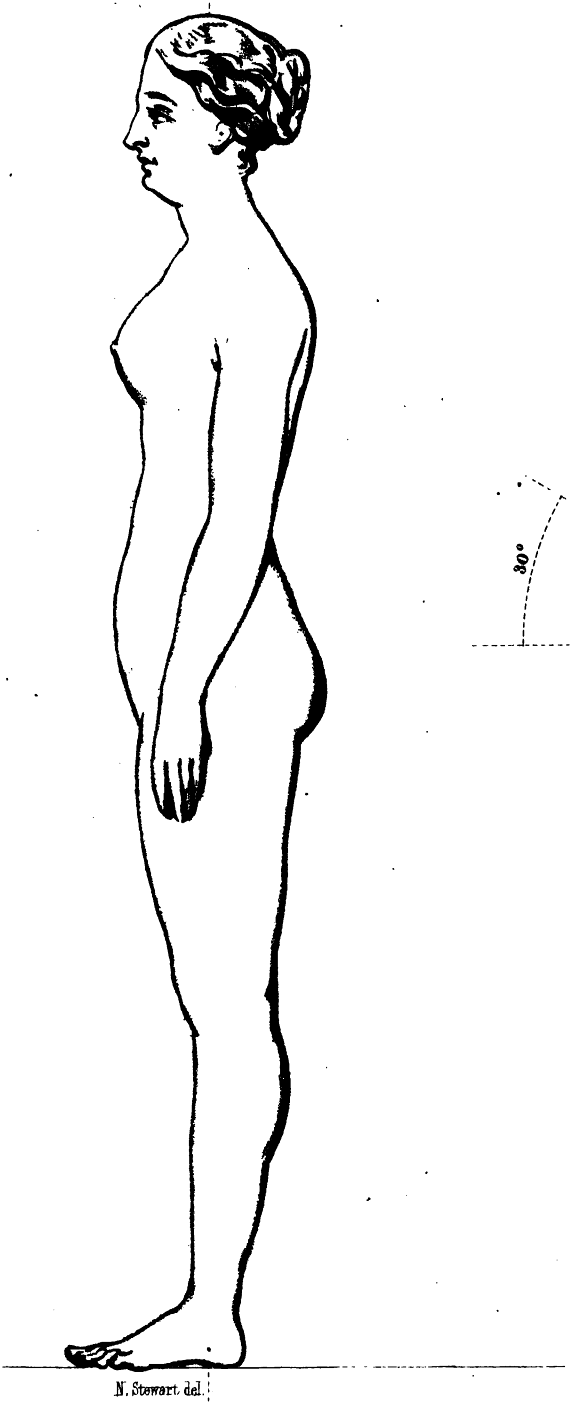
Dr Simpson, following Dubois, states this still more strongly. "The doctrine of gravitation" (says he), "presupposes that the mother's body is in the vertical or upright position, in order that the gravitation of the fœtal head may have the effect attributed to it. But during the hours of sleep and rest, her body is placed horizontally, and not vertically, and ought to afford many more chances than the statistical results shew, of the head falling by mere gravitation into other positions and localities, than its usual and normal locality in the cavity of the cervix uteri. Besides, most practitioners have repeatedly seen patients restrained to the horizontal position for months before labour came on, without this position of the mother producing any deviation from the common position of the fœtus. *The attitude and positions of the fœtus in utero.*—p. 7.

[2] "Whoever" (says William Hunter, *On the Gravid Uterus; Rigby's Ed.*, p. 6), "has any tolerable notion of the shape of the abdomen, and situation of the cavity of the pelvis, must understand that the axis of the uterus is very far from the perpendicular line, its lower end being turned backwards, and its upper end, in proportion, turned forwards. This obliquity changes with the attitude of the body, and from many other causes. When erect, the weight of the uterus presses the fore part of the abdomen into a greater rotundity, and then the axis of the uterus approaches nearest to the transverse or horizontal line; and in a recumbent position, the contrary happens from a similar cause."

"Owing" (says Montgomery, *Signs and Symptoms of Pregnancy*, p. 6), "to the oblique attachment of the pelvis to the spinal column, and the projection of the sacrovertebral junction coming in contact with the posterior surface of the uterus as it increases in size, and begins to ascend out of the pelvic cavity, that organ cannot rise perpendicularly, but its fundus is inclined forwards with its

Fig. 1

Female Figure in Erect Attitude.



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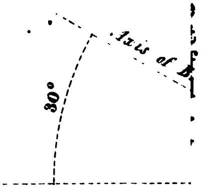

earlier direction. In the supine position, we find the uterus (see Figs. 7 and 8) to be not horizontal, but inclined to the horizon at an angle greater than that assumed in the erect attitude of the female. This angle is between 60° and 30°, probably much nearer 60° than 30°. In this way, it is evident that the uterus is more nearly vertical in the horizontal position of the woman, than in the erect. In both cases it is in an oblique position; and by this beautiful arrangement, the uterus is preserved, in an uniform condition, free from many statical variations which might otherwise affect it suddenly and injuriously.

The fact, that in none of the ordinary positions of the female is the uterus vertical, insures for the organ a considerable hydrostatical advantage. If the uterus were at any time vertical then the lower part of the organ, the part least supported and perforated by the os would be liable to be subjected to the pressure of a column of liquor amnii of about twelve inches in height, or equal to the longest diameter of the uterus. But by inclining the organ from the vertical position, the vertical height of the column of water is diminished, and with it the amount of the fluid pressure on every part of the walls of the organ.

Before leaving this portion of my subject, I wish to point out one practical application of it. In the third quarter of pregnancy, when the fœtus has acquired considerable firmness and weight, and before it has become at all closely adapted in form and dimensions to the uterine cavity, it may be made, by a very small amount of force, to bob about in the liquor amnii. Various positions of the female have been recommended as best adapted for successful repercussion or ballottement, as this bobbing of the fœtus is called. Numerous authors, especially those of the French school, advise the practitioner to place the woman in the erect position; others recommend the horizontal position on the side, on the back, with the shoulders raised, or on the knees and elbows. But a little attention to the configuration and attitude of the uterus will shew that, on mechanical grounds, there is one position decidedly preferable to the others. This position is that of course where the operation can be best accomplished; in other words, where there is most room and freedom for

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anterior surface lying against the peritoneal lining of the abdominal parietes —a relation of parts which continues unchanged throughout the whole period of pregnancy."

"In the erect position of the body" (says Robert Lee, *Lectures on Midwifery*, p. 91), especially if the abdominal muscles are relaxed, the fundus uteri falls forward, so that the axis of the uterus forms a great angle with the spinal column and brim of the pelvis, and approaches the horizontal line."

the motion. In regard to the uterus itself, this is evidently its long diameter in a vertical direction. Its long diameter offers most space; and if, in any other than a vertical position, motion in it would be impeded, not only by the liquor amnii, but also by the fœtus's own friction on the wall of the uterus to which it gravitated, and on which it would, to some extent, move. Now, in what position of the female is the uterus placed, with its long diameter vertical? We have already said that the organ developes itself forwards and upwards nearly in the direction of the axis of the pelvic brim in all the earlier months of pregnancy, at least. If, then, we place the brim, which, in the erect attitude, is inclined to the horizon at an angle of 60°, in a horizontal position (see Fig. 9), then the uterus will be vertical. For this purpose, the axis of the female, or at least the trunk, must be deflected to the same amount from the vertical line; that is, so as to form an angle of 30° with the horizon (see Fig. 9). Nægele, indeed, has pointed out that, in a position between that of sitting and recumbency, the superior plane of the pelvis will be horizontal in a well-formed woman. This can easily be effected, in actual practice, by propping the female with pillows on a couch. It is the position I have often found to be actually best adapted for the diagnosis of pregnancy by ballottement.

II.—Position of the Fœtus in Utero.

To give point to my description of the position of the fœtus, I may first state that it is often described as being the opposite of that of the adult. It is said to carry its body vertically, but with the head placed undermost; in other words, it is said to stand upon its head. To show that these descriptions are entirely wrong, it is merely necessary to observe that the fœtus at the full time is closely adapted to the cavity of the uterus, and to recall to mind what has been already said in regard to that organ. It will thus be observed that in the erect and supine positions of the female, the fœtus, near the full time, lies obliquely to the horizon, that in neither attitude is it at all nearly vertical or horizontal, but in a position about midway; and that in the erect position of the female (see fig. 4 and 6), it is more nearly horizontal than in the supine (see figs. 7 and 8). The mature fœtus is nearly horizontal, in ordinary circumstances, only when the woman lies flat on the side. It is vertical, only when the trunk is inclined to the horizon at an angle of about 30 degrees.

From this position of the adult fœtus in the axis of the

brim of the pelvis, several minor advantages might be pointed out as accruing to itself. But we shall here merely observe that this fact is an important element in the mechanism of parturition, the fœtus being thus placed in the direction most favourable for the uterine efforts to overcome the most important mechanical difficulty of labour, the passage of the brim.

In the same way, as we have found the nearest (though far from absolutely) correct explanation of the equilibrium of the gravid uterus to lie in the mechanical principles of the inclined plane, so shall we find these similarly applicable to the case of the fœtus. As we described the uterus to derive support on all sides from the organs environing it, so we find the fœtus to be much more perfectly supported by the bland and warm liquor amnii, in which it neither *stands* nor *carries* itself, but reposes floating in almost perfect equilibrium. It is, however, specifically heavier than the liquor amnii. It therefore sinks in this fluid,[1] and it does so with an almost unappreciable force. In accordance with the doctrines of the inclined plane, this minute force is subdivided between the plane on which it lies, the anterior wall of the uterus and the brim of the pelvis, the latter bearing the smaller share. The same hydrostatical circumstances of the floating fœtus which relieve the uterus almost entirely from the pressure at particular points of a body which might obstruct its circulation, or otherwise injure it, do also, in the same degree, relieve the child from pressure which might be painful or irritating to it. By this beautiful arrangement, also, the cord is safe from liability to pressure which might be fatal to it.[2] Again, the protection (says Dr Arnott[3]) given to the tender fœtus by the liquor amnii is such, that a blow from without is expended on the surrounding water and cannot reach the fœtus. During labour these same waters perform important functions, which belong, however, to hydrodynamics, not to hydrostatics.

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[1] At the full time sometimes so very little liquor amnii exists that it can scarcely be described as floating. The minuteness of the weight or force with which the fœtus generally presses on its supports can be understood by any one who has used the sign of ballottement. In performing this operation, the accoucheur, passing the finger per vaginam, generally succeeds in pressing the lower wall of the uterus against the presenting part of the child with some difficulty. With the point of his finger in this position, he jerks the presenting part of the fœtus. It immediately bobs up against the hand he has placed over the anterior abdominal wall, and drops down again on the point of his finger. If an accoucheur who has done this will try how he can move, with his hand similarly placed, a body weighing in air as many pounds as the fœtus does, he will then understand how nearly the fœtus must be in perfect equilibrium, and with how little of its weight it presses upon the uterus.

[2] I do not here enter into other provisions against this accident, the chief of which probably lies in the fact that the besoin de respirer, or the sensation of irritation analogous to it in the fœtus, excites it to motions which may relieve the cord from the asphyxiating pressure.

[3] Physics, vol. i. p. 688.

The head of the living fœtus, at and near the full time, is generally its lowest part or that nearest the brim of the pelvis. The frequency of this when compared with the other positions at or near the full time, is such that it claims to be called the natural position of the child in utero. The evidence on which this important statement rests is twofold; for, first, we can by vaginal examination at this time feel the fœtal head in that place, diagnosing it from other parts of the child; and, second, we know that, (to use the words of the admirable W. Hunter) in the last two or three months of gestation the child is commonly so much straitened for room, and so compactly adapted to the oblong figure of the uterus, that it cannot change its general position, either by its own efforts, or even by accidents happening to the mother.[1] It is on this last ground that accoucheurs do with justice adopt as an index of the numerical frequency with which the head lies lowest in the end of pregnancy, the ascertained numerical frequency of presentations of the head at the commencement of labour.

It is here necessary to make a digression in order to consider the statements of the distinguished M. Dubois and his numerous followers, in regard to the position of the fœtus in utero when not at or near the full time. These statements are founded upon statistics of the presentations of premature children in the course of miscarriage or abortion. They have been chiefly adduced in the writings of Dubois and others, who have attempted to disprove the gravitation theory of the position of the fœtus in utero. The statistics show that the earlier the fœtus is expelled from the uterus, it is the less likely to present the head as the first part born. But by an unwarrantable and quite unsupported step in reasoning they have been represented as proving the position of the fœtus in utero. These statistics are valuable, simply as indicating the presentation not before but only during parturition of premature children, and if such is the case they are utterly valueless in the discussion of the question of the position of the fœtus before labour, or of its causes, into which they have been dragged.

I have already shown why the presentation in early labour, of the fœtus, at or near the full time, is a very exact and trustworthy indication of its position before labour, and that, in many cases, we can easily verify the identity of the presentation in labour with the part lying lowest before labour.

[1] Gravid Uterus, p. 61.

But we have no evidence whatever that the presentation in a miscarriage or abortion, is identical with the part lying lowest before miscarriage, and we have no means whatever of reasoning from the one to the other. On the contrary, the more round and globular shape of the uterus in the middle months of pregnancy (see fig. 11), the non-adaptation of the child to the uterus, the great mobility of the child and the greater abundance of liquor amnii in these months, are some of the mechanical elements in these early stages of pregnancy, which render it almost absurd to reason from the presentation of a premature child during birth to its position in the uterus, before the process of its expulsion commenced.

There are no collections of ascertained facts in regard to the actual ordinary position of the fœtus in utero in the middle months of pregnancy. We can only reason in regard to it from comparing the position of the fœtal heart with the shape and site of the uterus, from feeling in some cases the position of the limbs or of the head in utero, from the questionable results of post-mortem examinations in these months, and from the statical knowledge which we can bring to bear upon it *a priori*.

It is not my intention, on this occasion, to enter into the whole subject of the causation of the attitudes and positions of the fœtus in utero. That would lead me into many questions very wide of a statical inquiry. My object at present is, merely to point out the great and predominating influence in their production, namely, that power of gravitation which all bodies, alive or dead, must obey.

I have already incidentally shewn, in the course of this paper, that the main arguments used against the gravitation theory are founded in error. When rightly considered, they either afford no argument whatever, or else, evidence against the side they are intended to support. These arguments are based on the erroneous notion that the gravitation of the head to the lowest part of the womb, can have effect only when the woman is upright, and is rendered abortive when she is horizontal. They also are made to derive support from the reasonings founded upon the erroneous belief that the lowest part of the child, during a miscarriage, was with certainty lowest before the miscarriage commenced.

Another specious argument adduced against the gravitation theory, requires only a few words to shew its imperfection. If gravitation (it has been said) were the cause of the normal position with the head lowest, then this position should be found with more certainty when the gravitation of the head, from any cause, was proportionally greater than natural. In cases of intrauterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is so to an excessive degree. But this increased gravitation (it is added)[1] does not render head presentations in these cases more common than usual, but the very reverse; as has been shewn by Dr Thomas Keith in his essay on this subject. But this argument, when justly applied, is made to tell most decidedly in favour of the gravitation theory. The altered circumstances of a hydrocephalic fœtus have been altogether misapprehended. The hydrocephalic head, although truly much larger and heavier in air, is probably lighter and more buoyant in water. In this question we have to do with hydrocephalic children only while immersed in liquor amnii. The larger the head is in these circumstances, the lighter and more buoyant it is, and the other extremity of the child is proportionally heavier. The fluid effused in hydrocephalus[2] is specifically lighter than brain, and therefore, renders the head more buoyant than it is under natural circumstances. It is also in all probability specifically lighter than liquor amnii, and consequently its accumulation will have a more decided effect in elevating the head. In this way it is demonstrated that the four times greater frequency of preternatural presentations in cases of hydrocephalic children, proves rather than disproves the influence of gravitation in deciding the position of the fœtus.

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[1] If the physical gravitation (says Dr Simpson) of the head of the child were the cause of the normal position with the head lowest, then this position ought to be found with more frequency and certainty when the gravitation of the head from any cause was rendered proportionally greater than natural; with less frequency and certainty, when from other causes the gravitation of the cephalic extremity of the infant was rendered proportionally less than natural. The very contrary, however, of all this is the truth. In cases of intrauterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is so to an excessive degree. But this condition of the head, this increased preponderance and gravitation of it, does not render head presentations in these cases more common than usual, but the very reverse. *Attitude and Positions of the Fœtus in Utero,* p. 9.—Extracted from Monthly Journal of Medical Science for 1849.

[2] I lately had an opportunity of procuring hydrocephalic fluid from a living child still in utero. I had drawn off the water in order to facilitate delivery; and I am indebted to Dr George Wilson, for a careful examination of the fluid. He found its specific gravity at 60° Fahr. to be 1007·9. The specific gravity of fœtal brain I do not know, but that of the adult varies between 1030· and 1050· *See Dr Skae's Paper on the Weight and Specific Gravity of the Brain in the Insane. Monthly Journal of Medical Science, October* 1854. In an experiment I lately made on a fresh fœtus at the full time, which had been exposed to the air for about 24 hours, I found the specific gravity of the entire body to be nearly 1050 at 44° Fahr.; and that of the head, when separated, to exceed that of the headless trunk. The specific gravity of liquor amnii of a premature ovum, is stated by Dr Rees at 1008·6 (*See Churchill's Midwifery, 3d Ed. p.* 93.) But it, no doubt, varies in different cases, and probably in the same case at different times.

Again, it has been argued that anencephalic children, with the whole brain and arch of the cranium wanting, are still often found presenting naturally, whereas it has been thought that this great deficiency should render presentations of the head less frequent than natural in these cases.[1] What is the fact, as to the greater or less frequency of the presentations of the head in these cases, I do not know. But whatever it may be, the argument is very inaccurately used. For even without the head altogether, the upper end of the trunk is the heavier. "Without descending to minutiæ"[2] (says W. Hunter) "it must be observed that the trunk is very small in proportion to the head, and the lower part of the body when compared with the upper; thus, the upper part of the trunk of the body is small with regard to the head, the lower part of the trunk is small in proportion to the upper part, and the lower extremities are small in proportion when compared with the arms."

I lately tried the experiment of floating in a solution of salt, of nearly its own specific gravity, a fresh decapitated fœtus, and found that it floated obliquely, with the neck very decidedly lowest. But before any argument applicable to the case of healthy pregnancy can be derived from observing the presentations of acephalous or anencephalous children, it is necessary to take notice not merely of the presentation in such cases, but the shape of the uterus, the quantity of liquor amnii, and of any abnormal circumstances in such abnormal pregnancies affecting the statical conditions of the fœtuses. Till this be carefully done, we can base no reasonings upon the presentations of anencephalous children whose statical circumstances we do not know.

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[1] The confused and erroneous reasoning of authors on this subject is well illustrated in their discussing the position of anencephalous monsters.

"Let us add (says Dubois, *Mem. de l'Acad., Roy., de Med., Tom. ii.* p. 270) that, if the laws of gravity had so great an influence upon the position of the fœtus, then the anencephalous deprived of the greater part of the brain should offer almost constant exceptions to the ordinary rule. Their head being much lighter, should, in fact, occupy the upper part of the uterus, and their pelvic extremity, drawn by its greater weight, should be approximated to the uterine orifice; but such is not the case. We cannot however deny, that the cephalic extremity of these fœtuses presents first in labour less frequently than is observed with those who are well formed. It is not impossible that the preponderance of the pelvic extremity may have some effect in these anomalies, but we are of opinion that it is only an accessory cause."

We find Dr Simpson agreeing with Dubois in this argument. He says (*Attitude*, &c., p. 8,) "If the physical gravitation of the head of the child were the cause of the normal position, with the head lowest, then this position ought to be found * * * * with less frequency and certainty, when from other causes the gravitation of the cephalic extremity of the infant was rendered proportionally less than natural * * Anencephalic fœtuses, with the whole brain and arch of the cranium wanting, are still often found presenting naturally. I have been present at the birth of three anencephali that had reached the full term of pregnancy. All of the three presented with the deformed and diminished cephalic extremity over the os uteri."

[2] On the Gravid Uterus, p. 62.

Further, it has been attempted by Dubois and others[1] to shew that the position assumed by the fœtus when immersed in a fluid, or rather when allowed to fall through a mass of water, is inconsistent with the gravitation theory. It has been shown, in these ill-devised experiments, that the part of the fœtus which reaches the bottom of the vessel first is not the head, as these authors fancy should be the case if the gravitation theory were correct, but is the shoulder. But the fact is, that the line of descent through a fluid of nearly equal specific gravity with the body descending, is in the case of the fœtus, as in all other bodies, not a vertical line in the long axis of the body, drawn from what is the absolutely heaviest part, but a vertical line from the centre of buoyancy to the centre of gravity. (See fig. 10.) The same fact is expressed in the hydrostatical principle, that a body floating and immersed is in stablest equilibrium when the centre of gravity is vertically below the centre of buoyancy, —*i.e.*, the centre of gravity of the fluid displaced. These very experiments, and numerous other facts, show that the centre of gravity is much nearer the cranium in the fœtus, and especially when in the fœtal attitude, than it is in the adult; that it is, in fact, near the upper part of the back, and nearer the posterior, than the anterior surface of the child. The centre of buoyancy of the fœtus will be very nearly the centre of the oval figure which encircles the fœtus. (See fig. 10.) Both these centres will be liable to constant variations with the varying dimensions and specific weights of different parts of different fœtuses, and with the varying attitudes of the parts of the same fœtus. But allowing for every variation, it is evident, that if these two centres be in the case of the fœtus placed in the same vertical line, the fœtus will be inclined obliquely to the horizon, as we find it to be in its ordinary position.[1]

The experiments of Dubois and Simpson, involving as they do, rapid motion in falling through water, are fitted to afford results of a very small degree of value or reliability. So far as they go, they point to the fact of the descent of the fœtus, shoulder lowest, or, to speak more correctly, following the

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[1] "When a human fœtus of the latter months," says Dr Simpson, "is placed experimentally in fluid, in descending through the fluid the head does not turn and fall first to the bottom of the containing vessel, as the theory of gravitation takes for granted that it would. Thus, if a dead new-born fœtus be plunged into water, contained either in a vessel shaped like the uterus, or in a large bath, the part which gravitates and strikes the bottom of the vessel or bath first, is

Fig. 10

Position of the adult Foetus in utero & of any similar body if supposed floating freely in a fluid of its own density.

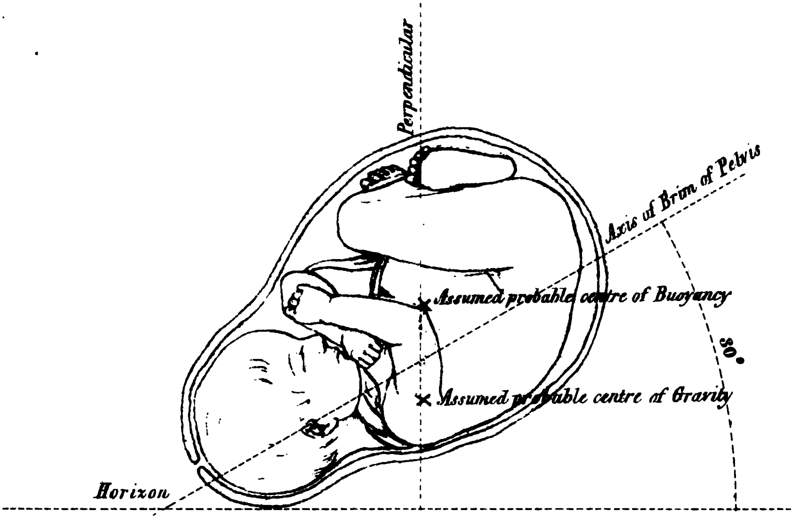
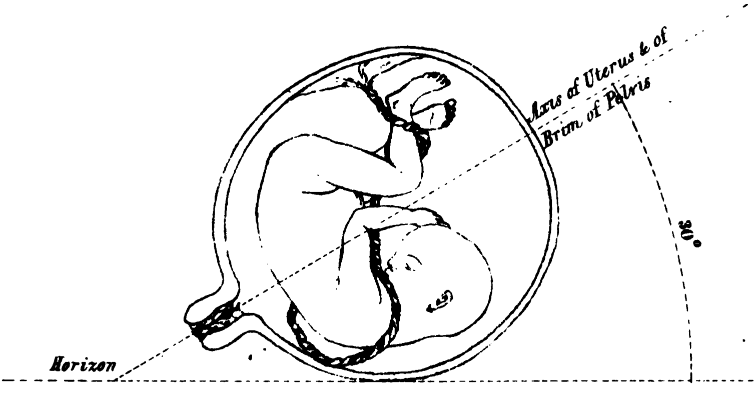

Perpendicular
Axis of Brim of Pelvis
Assumed probable centre of Buoyancy
30°
Assumed probable centre of Gravity
Horizon

Fig. 11

Uterus & Foetus about Middle of Pregnancy.



shoulder. In an experiment I lately performed with a fresh, still-born adult fœtus, retained by threads in the fœtal attitude, I found it to poise itself in an oblique direction, head lowest as in figure 10, in a solution of salt nearly its own specific gravity. This position evidently corresponds closely to the ordinary uterine position.

This argument against the gravitation theory, it thus appears, has been quite misapprehended, and a more correct investigation of the circumstances reveals to us here, as everywhere else, the beautiful arrangements of nature, whereby the fœtus reposes in its sojourn in the womb in that position which is easiest and securest to the mother and to itself,—in a position, indeed, which requires no effort whatever on its part for its adoption and maintenance.

The only other objection to the gravitation theory, which I think deserving of mention, is founded on the fact that dead or putrid children present the head as the lowest part in labour less frequently than the living; and it must be admitted that the death of the fœtus does not remove it from the agency of gravitation. From these propositions the conclusion is jumped at, that mere gravitation cannot account for this difference in ordinary position of the putrid and the living, for it is evident that gravitation should affect live just as it does dead matter. But here it has only to be pointed out that the major proposition of the syllogism is an assumption, and that this argument is a reasoning in the dark.[1] The assumption is one very highly improbable, namely, that a dead or rather a putrid child, is in the same condition, *quoad* gravity, as a living one.

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the back or scapula, and not the head. M. Dubois first pointed out this fact, as the result of numerous experiments which he had made on fœtuses from the fourth to the ninth month: and in several instances in which I have repeated the experiment, I have found the same result."

[1] Dr Simpson follows Dubois in this line of argument. I shall quote the words of the former in illustration of it.—*See Attitude and Positions, &c., p.* 9.

"When the child dies *in utero,* it still continues to be subjected to the same physical laws as when it is alive. The mere death of the fœtus does not in any way remove it from the agency of gravitation. When all its vital actions have ceased, the body should in fact be more subject than heretofore to all influences, such as gravitation, which are merely physical in their character. The dead infant ought, therefore, as frequently as the living, to have its head placed as the presenting part over the *os uteri,* provided the mere physical gravitation of the head were the reason and cause of that position. Experience however, amply proves that this is not the fact. In other words, experience proves that malpositions of the child, or the presence of other parts than the head at the cervix and os uteri, is in the latter months a far more frequent occurrence when the child is dead than when it is alive. . . . The position of the fœtus in utero, with the head lowest, is a vital action, &c. . . . What (he subsequently adds) is the nature and character of the muscular action by which the fœtus assumes and maintains its position in utero with the head lowest?"

We know that in the adult, at least, the circumstances of death and putridity produce great changes *quoad* gravity. What the changes are in the fœtus in similar circumstances we do not know. Till we do know, we must be content without attempting to reason as if we did.

Before proceeding with correctness to argue from the fact of frequent mal-presentations of dead children to the causes of the ordinary presentation of the head, we must take carefully into account not only the new circumstance of death in these cases, but also all other circumstances in pregnancies with dead children, which are, or may be, statically different from the circumstances of healthy pregnancy. Among these are prematurity, quantity of liquor amnii, the shape of uterus, and the shape of the fœtus. I have already said that those who use this argument have not directed their attention even to the altered statical relations produced by death and putridity of the fœtus. I regret to have only a single observation to adduce in advancing this question towards solution. So far as it goes it tells against the argument of the opponents of the gravitation theory. I floated in a solution of salt, of about its own specific gravity, a six months' fœtus, born dead, and with the skin peeling off. The position it assumed was exactly the reverse of that taken by the fresh child at the full time, and which I have already described. The putrid fœtus floated with its long axis directed obliquely to the horizon, and with its head highest. In labour it presented the breech.

The position of the fœtus at the full time is, in the great mass of cases, fixed and determined about the end of the seventh month of pregnancy. This arises from the fact that about that time the size and shape of the fœtus become so nearly and closely adapted to the size and form of the uterus, that it cannot change the position of its trunk in any material degree. After this time the position of the fœtus must be one determined by gravitation, for it is impossible to conceive its reposing in any other.

All the knowledge we possess of the position of the fœtus, after it has entered the second half of pregnancy, leads us to believe that its head lies ordinarily lowest. Before the seventh month it is still capable of having its position in utero changed, by changes merely in the attitude of the mother, and probably it possesses the power of effecting temporary changes, at least, by its own unaided movements. But the fœtus is generally in a state of repose, and not producing motions in its limbs or body. In this state of repose, in a fluid of nearly its own specific gravity, it is impossible to conceive of its maintaining any position but under the influence of gravity. Its position must at all times be mainly, if not entirely, caused and determined by statical circumstances. It is quite conceivable, that while still comparatively free in the uterus it may, by virtue of its very easy mobility in the dense liquor amnii, change its position. If this occur at a time when its dimensions are beginning to approximate to those of the uterus, having overcome some resistance of the uterine walls by the force of its own muscular efforts, or otherwise, as by accidents to the mother, it may not gravitate back to its old and ordinary position; and thus a preternatural presentation may be produced. But it is to be remembered that the same hydrostatical circumstances which give great comparative force to the muscular efforts of the fœtus, render its position in the same degree difficult of retention, and will in the same degree tend always to subject it to the influence of gravitation. The uterine walls are everywhere smooth and glabrous, and rounded; and the fœtus lies in this cavity with its legs, its chief organs of locomotion, elevated; circumstances which appear to render its maintenance of any position but that of gravitation a greater feat than ever was performed by a rope-dancer. With all the advantages of its new circumstances, the child after birth cannot assume or maintain any position. How much less could it be expected to do so in the uterus, and under circumstances so disadvantageous for the fulfilment of such a function. Those authors who, with Dubois, strive to prove that the position of the fœtus is determined by its own motions, have first to prove that it could maintain any position whatever against gravity, without such constant efforts as voluntary muscles are incapable of, and of the actual presence of which no evidence can be furnished.

III.—Position of the Pregnant Female.

The researches of Weber have shewn that in the erect attitude of both sexes in ordinary circumstances, the body is accurately balanced upon the two ilio-femoral articulations, the atlanto-occipital and sacro-lumbar articulations being in the same transverse-vertical plane, with the ilio-femoral articulations; the knee-joint and ankles being also in it (see fig. 2). This adjustment and balancing of the trunk upon the ilio-femoral articulations must, in the impregnated female, be gradually disturbed and changed more and more till the full period of pregnancy arrives. At this time the weight of the entire gravid-uterus and contents is superadded to that of the anterior parts of the body, which in the virgin state balanced the posterior parts upon the ilio-femoral articulations, the fulcrum of the pelvic lever. To restore the balance a corresponding amount of weight must be added to that upon the posterior arm of the pelvic lever. The weight added in front is probably about from twelve to fourteen lbs. avoirdupois,[1] and to restore the equipoise a corresponding amount of the upper part of the trunk and of the head must be moved backwards, so as to increase the weight behind (see figs. 4, 6, and 12). These theoretical observations must have been frequently confirmed by every observant eye in actual life. It is then to be remarked that the protuberant abdomen is rendered apparently more so by the attitude of the woman being as if excessively erect.[2] To the necessity for this new adaptation, the increased anterior development and weight of the mammæ will contribute. In small, and especially short-bodied women, this change is most evident, a circumstance easily explained by the greater prominence forwards in such of the uterine tumour, and by their not being able otherwise to adapt the trunk to the new circumstances, as can be done with greatest facility in tall or long-bodied women. Some few women, especially those last alluded to, are enabled to conceal to a certain extent their gravid condition by not assuming this change of attitude, nature securing by a different mechanism (see fig. 13), the equipoise of the trunk upon the lower limbs.

In the mechanism of the erect position, the centre of gravity must be vertically above the ilio-femoral articulations, or points of support. (See Figs. 12 and 13.) In the virgin this centre resides in the lower part of the lumbar division of the spinal column. The addition of the weight of the gravid uterus, and contents, in front, would move this centre forwards. This

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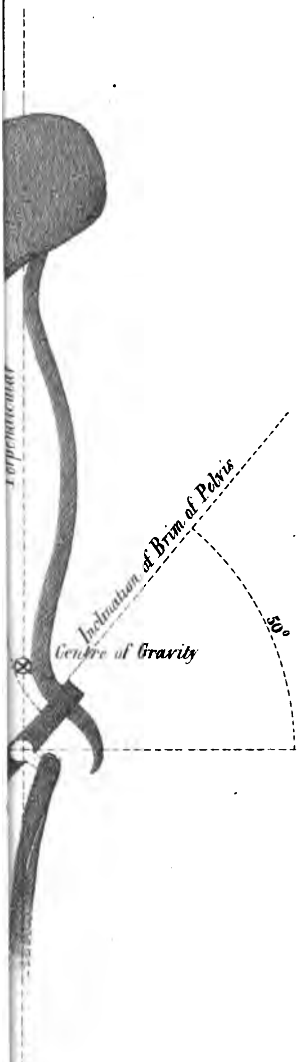
[1] Ramsbotham (*Obstetric Medicine*, p. 82,) calculates the weight of the contents of the gravid uterus at between nine and ten pounds. In accordance with this notion, I have drawn up the following approximative statement of the probable average weight of the gravid uterus and contents :—

	lbs.	oz.
Fœtus, ...	7	0
Placenta, Cord, and Membranes,	1	5
Liquor Amnii, ...	1	8
	lbs.	oz.
Uterus, as weighed post partum,	2	0
Blood in Uterine Sinuses, &c., say,	1	0
	12	13

[2] "From this obliquity of the uterus (says Montgomery, *Signs and Symptoms of Pregnancy*, p. 7,) the direction of the centre of gravity is changed, and instead of falling between the feet, it falls in front of them, in consequence of which the person has an inclination to fall forwards, and in order to prevent this is under the necessity of throwing back the head and shoulders, and assuming that pompous air which is so often unjustly attributed to a wish to make a display of her condition."



g. 13

adaptation of
rect attitude_Centre of
isplaced forwards.

Schenck & M^c Farlane Lith^{rs} Edin^r

is prevented, however, in most cases by the corresponding motion backwards of the upper portion of the trunk. In some women, either from voluntary or involuntary causes, this last motion does not take place. The equipoise of the body upon the ilio-femoral articulations, or restoration of the centre of gravity to its position vertically over these points of support, must be otherwise effected. This can evidently be done only by moving forward the supports, so that they may be vertically under the new site of the centre of gravity, in this class of pregnant females. But as the acetabula are fixed bony sockets, this can be done only by diminishing the angle which the pelvis forms with the horizon. (See Fig. 13). In this diminution of the obliquity of the pelvis, the forward movement of the ilio-femoral articulations takes place, and the equipoise is restored without any backward motion of the upper part of the trunk. This change in the pelvis is analogous to that taking place in old age, when the forward stoop is counterbalanced by it. The same is also observed in youth, when a forward stoop is produced, as in the course of some affections of the vertebræ.

I have described these two distinct mechanical arrangements for the equipoise of the pregnant female, as occurring in different classes of pregnant women, merely to facilitate description. It is evident that in any case a certain amount of the one may be adopted as the complement of the other. But in many women the distinction and separation appears really to be maintained. In casually observing numerous pregnant females in this point of view, I have remarked two very characteristic classes. In the first class, the women are generally not tall in stature, have the upper part of the body much inclined backwards, and the hips also prominent, indicating a probably considerable obliquity of the pelvis. In the other class, the women if not tall are generally slender. In them, the upper part of the trunk does not appear to be inclined backwards, and the hips are generally flat.

In quitting this subject, I would wish to remark that in framing our views as to the obliquity of the pelvis, we must be guarded against the error of arriving at conclusions too absolute and strict. For, it appears to me that the inclination of the pelvis is liable at all times to vary in the same manner, as I have described it sometimes to do in pregnant females. Moreover, the observations of Zaglas, Wood, and others, shew that the innominate bones are far from being immoveably fixed to the sacrum,[1] and are capable of certain motions upon it, which are capable of being turned to advantage in establishing the equilibrium necessary for the erect position.

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[1] See Dublin Quarterly Medical Journal for August 1854, for a full discussion of this subject.

The last statical consideration, which I shall dwell upon, in connection with the position of the pregnant female, is one of practical importance, and which may be turned daily to account in actual practice.

I have already pointed out that the attitude of the gravid uterus is inconsiderably changed by the woman shifting from the erect to the supine posture, or *vice versa.* But though this be the fact, as regards the mere attitude of the uterus, yet great changes take place in it, in regard to its liability to vascular congestion, and other phenomena connected therewith. When the woman is recumbent, the blood is easily returned to the heart from the uterine sinuses, and there is little danger of vascular engorgement taking place. When the woman stands the return of blood is not so easily effected, it having now to overcome the increased resistance involved in the change of the position of the vena cava from being nearly horizontal, to being nearly vertical. Of this circumstance clinical illustrations abound in the practice of obstetrics. A woman of delicate fibre, and easily made to miscarry, may require to maintain the horizontal position for the whole, or a part only of the period of pregnancy. In some such cases the assumption of the erect attitude for the shortest time induces engorgement of the uterine sinuses, separation of the placenta, hemorrhage, and abortion, or miscarriage. Hemorrhage, again, is induced in some cases of ulceration, and polypus, only by continuation in the erect attitude; in the same way colour often re-appears in the discharges after delivery, on the first getting up; and all uterine hemorrhages are aggravated by the erect attitude. To the same cause also are to be attributed many inflammatory affections of the uterus, and many of the uterine pains attending disease in this region.

ART. V.—*Medical Notes on the Climate of Burmah, and on the Fevers and other Diseases which have there prevailed among European Troops.* By CHARLES MURCHISON, M.D., Acting Physician to the Westminster General Dispensary, and late of the Bengal Medical Service.

Perhaps there are few subjects on which so much has been written as that of fever, and yet there are few in which there yet remains so much room for investigation and research. This observation applies most particularly to the fevers of tropical climates, regarding which the most vague and varied opinions are entertained by many, and as to the pathological nature of which but a very imperfect knowledge is possessed by any. One cannot avoid being forcibly struck with the truth of this remark, who has visited our Indian possessions, observed the various forms of fever which there prevail, listened to the opinions, and watched the practices of different medical men. With regard to one and the same fever, the most opposite opinions may sometimes be found to be entertained, and the most opposite modes of practice put in force. Surely, then, notwithstanding the admirable writings of Annesley, Twining, Johnson, Martin, and others, our knowledge of tropical fevers requires to be placed on a better footing, and the subject still offers a wide field for research, while, at the same time, it is one of the highest importance, if we consider, that every European who makes any sojourn in India, suffers more or less from fever, and that so many fall victims to it.

It is not the object of this paper, however, to enter into any general discussion on tropical fevers. What we now want is not a complication of theories and opinions, but a series of carefully observed and recorded facts, and now that we have an "Indian Annals of Medical Science," it is to be hoped that our medical brethren in the East will, from their ample opportunities, contribute more than hitherto, to the advancement of our knowledge on these and other subjects. Believing, then, that it is the duty of every one to contribute what he can towards the advancement of his profession, I have ventured to lay before my medical brethren the following observations on the fevers and other diseases, which have been prevailing among our troops in our newly-acquired possessions in Burmah; these observations having been derived from cases occurring among the Europeans of the Second Bengal Fusiliers, during the three months after their first arrival in Burmah, but equally applicable to the diseases which prevailed in other European regiments. These observations, too, are the more important, as it is always interesting to observe the diseases, with which strangers are attacked on their first immigration into a country, in reference to which we know but little.

The unhealthiness of the climate of Burmah has long been notorious, so much so, that the "pestilential swamps on the banks of the Irrawaddy," have become almost a byeword, and a byeword not altogether undeserved, when we consider, that some of our regiments lost upwards of 26 per cent. of their strength in the space of one year, partly from the accidents of warfare, but principally from the baneful effects of the climate. Mr Taylor, the surgeon of H. M. 80th, informs us, that the mortality in his regiment during twelve months in Burmah, was 26·5 per cent., or more than four times the average ratio of mortality during nearly eight years of previous service in India;[1] and in H. M. 18th, Royal Irish, according to Mr Stewart, out of a strength of 1088, there were 1624 admissions into hospital in twelve months, and 260 deaths, or 23·9 per cent.[2] It will be my object to point out some of the causes of the great sickness which has prevailed; but I may here remark, that very possibly the climate of Burmah may yet prove not so peculiarly unhealthy as it is generally believed to be; and that, in all probability, there would have been an equal amount of sickness and mortality, among European troops, had they been exposed in many parts of our Indian territory before subjection to European civilization and improvement, in the way in which our troops have been exposed in Burmah.

The following observations may be classified under five heads:—First, we shall say a few words on the geological and geographical position of Prome, and on the climate, more particularly of the season, in which the 2d Europeans arrived there. Secondly, we shall mention a few particulars connected with the arrival of the 2d Europeans at Prome. Thirdly, we shall enumerate the various diseases which prevailed among them, after their arrival; and, fourthly, the causes to which the great prevalence of disease may be attributed. In the fifth and last place, we shall enter into a few particulars with regard to a few of these diseases, more particularly the fevers.

I. First, then, we shall consider the geographical and geological position of Prome, and say a few words on the climate. Prome in lat. 18° 48′ North, and 95° 10′ East longitude, is built on the left or eastern bank of the Irrawaddy, a little below the point where this is joined by the Nawaing river, about 300 miles above Rangoon, and 360 miles below Ava, and 40 miles from the present frontier of the British possessions in Burmah. The country, for several hundred miles above Rangoon, consists of one vast alluvial plain, intersected by rivers, by which it is inundated every rainy season, and covered with rice fields or dense jungle. This plain is continued up along the banks of the river to within 30 miles of Prome. Here, at a place called Sahlaydan, on the western bank of the river, a range of hills appears, a branch of the great Arracan range, which is continued up along that side of the river to above Prome, while a little farther up, on the opposite bank, another range commences, continuous with the hills, which, from their terminating in the immediate vicinity of Prome, generally receive the name of the Prome mountains. These consist of a number of undulating hills, the highest point, "Boxer's Hill," being about 200 feet above the level of the river. The hills on the opposite side are much higher, some of them exceeding 400 feet. The Prome range, at whose northern extremity the town of Prome is built, extends from the river's bank eastward for about two miles, and is then succeeded by an alluvial plain, stretching uninterruptedly to the base of the Galladzet Mountains, a distance of about 30 miles. This plain, which in a southerly direction, is continuous with the great alluvial plain of the Irrawaddy already mentioned, is covered with jungle, extensive rice fields, and a few teak forests, and during the rainy season the greater part of it is under water, the only mode of communication between many of the towns and villages being by means of boats; while the protracted dryness after the rains breaks up the rice fields by extensive fissures, often two or three feet in depth.

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[1] Indian Ann. of Med. Sc. I., p. 409.

[2] Indian Ann. of Med. Sc. I., p. 430.

The hills at Prome consist of a shingly sand, lying in strata at an inclination of about 45°, with a dip to the eastward. Some of the strata contain a considerable quantity of argillaceous matter, and often form a marked contrast to those immediately above and below them. This sand, which probably belongs to the Diluvium, afterwards alluded to, appears to lie upon a formation of a granular, sandy, yellow limestone, arranged in strata, also at an inclination of 45°, with a strike north and south, and containing a quantity of marine shells and other organic remains very similar to those of the London clay[1]. This formation is best seen in the hills opposite to Prome, but large blocks of the rock are also found on the Prome side, along the bank of the river, between Prome and Shoaydong[2]. In the Prome hills there is also found a soft green, and yellow sandstone, and I have also observed a sandy clay, which presents the peculiar honey-combed appearance, constituting the mineral Laterite[3].

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[1] The following shells, &c., found in this rock have been identified by Mr Sowerby, with those found in the London clay, and *Calcaire grossier* of Paris, Ancillaria, Murex, Cerithium, Oliva, Astarte rugata, Nucula rugosa, Teredo., Teeth of shark, fish scales, &c.

[2] A town 8 miles below Prome.

[3] This mineral has been stated not to exist at Prome. Ind. Ann. Med. Sc. I. 389.

Professor Buckland,[1] from a number of geological specimens brought from Burmah, in the year 1826, by Mr Crawfurd, who had accompanied the British embassy to Ava, endeavours to make out the following geological formations in Burmah. 1. *Alluvium*, formed by deposits from the river, and constituting the extensive plain, already alluded to. 2. *Diluvium*, seen principally in a district, 20 miles square, at Yay-nan-gheoum, about halfway between Prome and Ava, where the celebrated Petroleum Wells are. This formation consists of a sand with a coarse gravel, containing the fossil remains of numerous vertebrate animals. Among these Mr Clift[2] has described those of two new species of mastodon, and also remains of the following genera, hippopotamus, sus, rhinoceros, tapir, ox, deer, antelope, trianyx, emys, leptorynchus, and crocodile. Remains of vertebrate animals I have often also observed in the coarse shingly sand, forming the hills of Prome, in the deep cuttings through these hills, which our Indian Government has lately been making for the construction of roads. Along with these animal remains is also found large quantities of fossil wood, both monocotyledonous, and dicotyledonous. Portions of this fossil wood are often washed out from their bed by the rain and torrents, and carried down the river, on the banks of which they are often found, many miles below their original locality. The natives of Burmah regard this wood with a certain degree of veneration, and often transport it to immense distances, for the purpose of employing it as pillars of support for their religious edifices. This practice probably constitutes the true explanation of the statement made by Lieutenant Alexander, that the waters of the Irrawaddy had the power of converting wood into stone in the space of 30 months,[3] about which there was formerly some discussion[4]. 3. *Freshwater Marl*, a marly blue clay, containing numbers of a species of Cyrene, and found at Yay-nan-gheoum. 4. *London clay and calcaire grossier*, already alluded to as found at Prome. Numerous marine shells have also been found in a dark-coloured slaty limestone, near the Petroleum wells. 5. A soft green, and yellow sandstone, corresponding to our *Plastic clay formation*, found at Prome, and Yay-nan-gheoum, and at the latter place, like our clay, associated with brown coal, and petroleum. 6. *Transition limestone*, found at Kyouktaran, near the commencement of the alluvial plane, and at Toungbaran, and Loonghee, on the left bank of the river between Prome and Ava. 7. *Metamorphic* rocks are found in the Sakaing chain of mountains, a little to the north of Ava, such as mica slate, hornblende, and statuary marble. An immense quantity of this last rock is made use of by the Burmese, for making images of their great object of worship —*Godama*, an incarnation of Budh. It is very white, has a beautifully crystalline structure, and is susceptible of a high polish.

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[1] Trans. Geol. Soc. Ser. 2. vol. ii. p. 377.

[2] Ibid., 2. vol. ii. p. 369.

[3] Edinburgh Philosophical Journal. 1828.

[4] "On the power of the Waters of the Irrawaddy to convert wood into stone." Trans: Geol. Soc. Ser. II. vol. ii. p. 394.

The native town of Prome is built on the plain at the northern extremity of the Prome mountains, along the banks of the river, and here also, originally, were the quarters for our troops. Before the arrival, however, of the 2d Fusiliers, barracks for their accommodation had been built along the summits of the hills, sites, the propriety of selecting which, is very doubtful.

As to the climate of Prome, the seasons may be said to be two, the wet and the dry. The rains commence about the middle or latter end of May, and terminate about the commencement or middle of October. In 1853 they terminated on the 22d of October; in the preceding year, by the end of September. Extending, as the rainy season does, over a period of about five months, the quantity of rain that falls at Prome is not great, the whole annual fall (1853), as ascertained by Colonel Turton of the Bengal Engineers, not exceeding 30 inches; being less, in fact, than in many parts of Great Britain. At Rangoon the annual fall is more than double that at Prome, Dr Fayrer's observations showing that it there amounted (1853), to 81·25 inches. The average temperature in the rainy season is about 80°. During the whole season, the air is extremely damp; in fact, almost saturated with moisture. Everything feels wet, cloth clothes become covered with mildew, leather-work with mould, &c. The following are the results of observations made, partly by myself, and partly by Dr Anderson, of the Bengal service, during the dry season of 1853-54. The dry season, extending from October till May, presents some varieties. Heavy dews and fogs towards morning are extremely common, during the latter part of October, November, and December, sometimes lasting till 8 or 9 A.M., and completely obscuring the sun's rays. These, however, ultimately break through with great power, so that, even at this period, we have nothing approaching to the cold season in Bengal; for, though the thermometer may sometimes fall to 56° Fahr. in the morning, yet the same day it may rise to between 80° and 90°. These fogs at Prome were observed to lie along the earth's surface, and principally in the valleys, between the different hills. Thus, on looking down from the summit of a hill into an adjoining valley, sometimes nothing but a mass of dense fog could be seen; while, on the summits of the hills beyond, objects could be distinguished with perfect ease. About the middle of February the temperature increases, and the nights and mornings become warmer. The months of March, April and beginning of May, are extremely hot, and dry; the thermometer, in the day time, ranging from 90° to 110° in the house, and the atmosphere being so dry, that wooden furniture cracks, and warps; the divisions at the point of a quill pen fly asunder, vegetation is burnt up, and respiration often becomes a labour. At this period, hot dry winds from the north west are not uncommon, and the nights are very close and unpleasant. The following table shows the mean, maximum, and minimum temperatures, both in the sun and shade, for the latter part of October, and the months of November, December, January, February and March 1853-54.

TABLE I.

Monthly Temperatures at Prome.

MONTHS.	In the Shade.			In the Sun.		
	Max.	Min.	Mean.	Max.	Min.	Mean.
	°	°	°	°	°	°
October, from the 24th,	91·5	69	81·46	120	112·	116
November,	90	58·5	77·339	120	109	115·6
December,	86	56	73·85	117	102	111·74
January,	88	58	75·196	121	98	108·58
February,	98	57	78·48	125·5	95	114·6
March,	106	63	84·	127	112	120

The next table shows the maximum, minimum, and mean weekly temperatures in the shade, for the 22 weeks, extending from the 24th of October to the end of March.

TABLE II.

Weekly Temperatures at Prome.

WEEKS.	Maximum.	Minimum.	Mean.
October 1.	91°·5	69°·	81°·46
November 2.	90·	74·	81·12
... 3.	86·5	68·	78·41
... 4.	85·5	58·5	74·98
... 5.	83·5	59·	75·19
December 6.	86·	65·	75·99
... 7.	80·	61·5	73·94
... 8.	79·5	56·	72·08
... 9.	83·	57·	73·29
... 10.	83·	62·	74·47
January 11.	86·	63·	76·21
... 12.	86·	61·	74·55
... 13.	87·	59·	74·33
... 14.	88·	58·	76·05
February 15.	88·	59·	76·74
... 16.	90·	57·	76·75
... 17.	92·	63·	77·05
... 18.	98·	64·	83·64
March 19.	100·	63·	84·5
... 20.	199·	66·	85·
... 21.	102·	70·	85·42
... 22.	106·	74·	88·7

The preceding tables show, in the season 1853–54, a gradual decrease in the temperature, from the end of the rains in October till the second week of December, when it was at its minimum,—the mean weekly temperature for this week being 72·085°; and, from this time, it again progressively increased. The lowest temperature observed during the season was on the 19th of December; on which day, at 5¼ A.M., the thermometer stood at 56° Fahr. In the season preceding (52–53), the coldest day was observed by Mr Taylor to be the 2d of February,—the thermometer on the morning of that day being at 55°. From the middle of February, the temperature is seen to have increased very rapidly. With regard to the influence of the temperature on health, two points deserve to be noticed.

The one is, that there is no "cold season" at Prome approaching to that of Bengal and most parts of India. This may be illustrated by contrasting the mean temperature of the two coldest months—December and January—at Prome, with that of Calcutta and Benares, whose latitudes do not differ by many degrees.

TABLE III.

Mean Temperature of December and January at Prome, compared with that at Calcutta and Benares.

MONTHS.	Prome, Lat. 18° 48′ N.	Calcutta, Lat. 22° 23′ N.		Benares, Lat. 25° 20′ N.
		[1]	[2]	[3]
December, . . .	73°·85	68°·6	66°·6	62°·49
January, . . .	75·196	66·	66·	67·53

True, as we have already seen, the thermometer at Prome may occasionally fall to 55°, before sunrise, but the same day it may rise to between 80° and 90°. Were it not that the temperature was sometimes so low in the morning, the mean monthly temperature would be much greater even than it is in these months.

This great alternation of temperature in the course of the 24 hours is the other point to which I wish to call attention; and this, no doubt, contributes greatly to the unhealthiness of the climate. A few examples may render it more striking.

TABLE IV.

Daily Alternations of Temperature at Prome.

DATE.	In the Shade.			In the Sun.	
	5½ A.M.	3 P.M.	Alternation.	Maximum.	Alternation.
October 25.	69°	90°·5	21°·5	106°	37°
November 22.	59	80·5	21·5	114	55
December 19.	56	78·	22·	116	60
January 24.	58	88·	30·	107	49
February 27.	64	95·	31·	114	50
March 27.	72	102·	30·	125	53

[1] Henry Traill, Esq., 1785. Trans. As. Soc. Bengal, vol. ii., 1790.
[2] Dr Johnson. On Tropical Climates, p. 20.
[3] James Prinsep. Trans. As. Soc. xv., 1825.

If we contrast this alternation with that of most parts of India, we observe a remarkable difference. We find in a "Meteorological Diary"[1] kept at Calcutta for 23 months by Mr Henry Traill, that the greatest alternation of temperature *for the whole month* never exceeded 26°, and sometimes was not more than 9°; and again, from observations made on the temperature of Benares by Mr James Prinsep,[2] the greatest *monthly* alternation never exceeded 26·5°, whereas at Prome we have the *daily* alternation sometimes amounting to 31°. It may also be mentioned, that the heavy dews and damp mornings in November and December rendered the atmosphere even more chilly than would have been expected from the thermometric indications; and the contrast between the cold of the morning and heat of the day was often very striking.

The atmosphere, which for some weeks after the cessation of the rains was extremely damp, gradually became drier. This was confirmed by observations made with the wet bulb thermometer. Thus, during November the difference between the dry and wet bulb thermometer at the hottest part of the day seldom exceeded 20°, while in March it often amounted to 35° or 40°. At Rangoon the atmosphere is much more moist, and hot dry winds are unknown.

I have no record of barometric observations, but, generally speaking, the barometric range was trifling.

The prevailing winds during the dry season were north-east and north-west.

II.—We shall now mention a few particulars connected with the arrival of the 2d Fusiliers at Prome. This regiment, upwards of 1000 strong, before proceeding to Prome had been stationed at Fort William, Calcutta, but in the month of March four companies (about 400 men) were sent across the Bay of Bengal to Moulmain, near the north extremity of the Tenasserim coast, where they remained until the rest of the regiment was ordered to Burmah. The regiment may be said to have arrived at Prome in three different detachments. It will be necessary for me to say a few words with regard to each of these, as it will afterwards be seen that there was a marked difference between the three detachments, in reference to the prevalence of disease after their arrival at Prome.

The first detachment, consisting of Nos. 4 and 10 companies, amounting to 200 men with 6 officers, left Calcutta on the 1st of October 1853, and, after a very rough and tedious voyage, during which the men had to lie upon the deck of the steamer, exposed to the rain and sea-spray, arrived at Rangoon late on the evening of the 9th. On the 10th, thinking they were to remain at Rangoon, a number of the men were employed as fatigue parties in removing their baggage from the river up to the town, a distance of two miles, under a burning sun. On the 11th others were similarly employed in conveying their baggage back again to the river, to be put on board one of the river steamers. On the morning of the 12th they embarked in an open "flat" boat in tow of a steamer. The 200 men were crowded into the two decks of this flat, where they had but little protection from the rain, which occasionally fell in torrents, especially during the night. Hardly a night passed that many of them were not drenched by the rain, and in addition to this they were tormented by myriads of musquitoes of the largest and most irritating description, which entirely prevented sleep. In one part of the river, known as the "Pannaling Creek," these are so numerous, as actually, at certain hours of the day, to darken the atmosphere, and their bites were so irritating, that I have seen large ulcers result from them, and one of the men, who threw himself overboard and was drowned, was generally supposed to have done so, in consequence of the extreme annoyance he was suffering. Eleven days were they subjected to these hardships, reaching Prome on the 22d of October, the very last day of the rains. Here they were quartered in barracks, built along the tops of the hills, of wooden poles covered over with thin thatch, and with walls of thin matting, which afforded but little protection from the intense heat during the day or from the cold during the night. It may also be mentioned that this detachment left Calcutta in such haste, that they brought no blankets, and but little bedding with them.

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[1] Trans. Asiat. Soc. Bengal, vol. ii.

[2] Ibid., vol. xv.

The second detachment, consisting of Nos. 5, 7, 8 and 9 companies, amounting to about 400 men, which for some months had been stationed at Moulmain, arrived at Prome on the 28th of October. They had had no fatigue work, were only five days in coming up the river to Prome, and had no rain. They had blankets, but old ones. During their stay at Moulmain, they had not suffered greatly from sickness, but had become considerably debilitated from the effects of the climate and other causes.

The third or remaining portion of the regiment (Nos. 1, 2, 3, and 6 companies, with the head-quarters), amounting to about 400, left Calcutta on the 17th of October, had a good passage across the Bay to Rangoon, where they rested for upwards of a week, and then proceeded up to Prome, arriving there on the 11th of November. They had fine weather during all their journey, and had their full supply of bedding.

III.—We now proceed to consider the diseases which prevailed in the regiment, after its arrival at Prome. In order to give a correct idea of these, we have condensed the following table from the monthly hospital reports.

TABLE V.

Admissions into the Hospital of the 2d E. B. Fusiliers at Prome during November and December 1853, and January 1854.

Strength at Prome, varied from 531 to 425.	DISEASES.																																			
	Apoplexy.	Bubo simplex.	Catarrh.	Cephalalgia.	Cholera biliosa.	Cholera spasmodica.	Colic.	Constipatio.	Contusio.	Cynanche tonsillaris.	Debilitas.	Delirium tremens.	Diarrhœa.	Dysentery.	Dyspepsia.	Ebrietas.	Epilepsy.	Febris Contin.	Feb. Intermit. Quotid.	Feb. Int. Tertian.	Febris Remittens.	Gonorrhœa.	Hemorrhoids.	Hepatitis.	Lumbago.	Morbus Cordis.	Ophthalmia.	Orchitis.	Phlegmon.	Pleuritis.	Rheumatism.	Sciatica.	Syphilis prim.	Do. consec.	Ulcers.	TOTAL.
Admitted, ...	1	2	1	1	1	3	5	6	2	1	3	1	18	20	6	3	2	4	178	41	105	1	1	5	1	2	3	3	1	1	8	2	1	1	1	435
Discharged, ..	...	2	1	1	1	2	5	6	2	...	...	1	16	11	6	3	1	4	163	40	97	1	1	1	..	1	3	2	...	1	5	1	1	1	1	381
Died, ...	1	...	...	...	...	1	...	...	...	...	...	...	1	5	...	...	...	...	...	...	5	...	...	1	...	...	...	...	...	...	...	...	...	...	...	14
Remaining,...	...	...	...	...	...	...	...	...	...	1	3	...	1	4	...	...	1	...	15	1	3	...	...	3	1	1	...	1	1	...	3	1	...	...	...	40
TOTAL, ...	1	2	1	1	1	3	5	6	2	1	3	1	18	20	6	3	2	4	178	41	105	1	1	5	1	2	3	3	1	1	8	2	1	1	1	435

The table shows the number of men admitted into hospital at Prome, during the months of November and December 1853, and January 1854, the diseases for which they were admitted, and the results of the treatment.

It is to be observed, that in the table, under the head of strength, the whole regiment is not accounted for. This is owing to the fact, that shortly after the arrival of the regiment at Prome, four companies, Nos. 1, 2, 3 and 5, were sent down to Shoaydong, a town also on the left bank of the Irrawaddy, eight miles below Prome, where they had a separate hospital, while again a party of 250 men, partly taken from the head-quarters, and partly from the companies at Shoaydong, was sent into the Tharawaddy district on detached duty on the 26th of December. The numbers in the table show the strength of the regiment at Prome on the 1st of December, and the 1st of February, and if we assume the average strength during the whole of that period to have been 500, which is considerably above the mark, we find that, in the space of three months, 435 out of 500, or eighty-seven per cent, were admitted into hospital.

It has been already observed, that there was a remarkable difference in the prevalence of disease in the various companies of the regiment, during the first few weeks after their arrival at Prome. This will be at once obvious by reference to the following table shewing the number of men admitted into hospital at Prome from each company, from the arrival of the regiment, up to the departure of the above-mentioned detachment into the district, on the 26th of December.

TABLE VI.

No. of Company.	No. admitted into Hospital.	No. of Company.	No. admitted into Hospital.
I.	5	VI.	37
II.	3	VII.	38
III.	2	VIII.	19
IV.	77	IX.	43
V.	3	X.	72

It is to be observed with regard to this table, that the numbers placed opposite Companies I., II., III., and V., are hardly correct, as after the removal of these four companies to Shoaydong, they had a separate hospital, with the exact statistics of which we are unacquainted. The number, however, of sick in these companies, up to the 26th of December, we know to have been exceedingly small, and we feel sure, that by estimating an average of twelve admissions into hospital, for each company, during the whole period, we are far above the real number.

Now, if we consider this Table in reference to the order of arrival of the different companies, we at once perceive the truth of a previous remark, that there was a striking difference in the amount of disease in the three detachments in which the regiment arrived at Prome. Thus, it will be remembered the 1st detachment consisted of Nos. 4 and 10 companies, the second of Nos. 5, 7, 8, 9, and the 3d of Nos. 1, 2, 3, and 6. The following Table shows the numbers admitted from each of these detachments, from the date of their arrival up to the 26th of December :[1]—

TABLE VII.

Detachment.	Companies.	Strength.	No. admitted into Hospital.	Per centage.
I.	4 & 10	200	149	74·5
II.	5, 7, 8, 9.	about 400	107	26·7
III.	1, 2, 3, 6.	400	73	18·25

Thus, we see, that the per centage of sick in the first detachment, was nearly three times as great as that of the second, and four times as great as that of the third. It may be observed that the great sickness in the first detachment did not break out immediately on their arrival. Only four admissions into hospital took place during the first week of their stay at Prome.

With regard to the detachment which marched from Prome into the Tharawaddy district on the 26th December, it may be interesting to mention a few particulars. The force which left Prome on this date, consisted of 250 men and 6 officers of the 2d Europeans, 40 men and 2 officers of the Madras European Horse Artillery, and detachments of various native regiments. We shall confine our remarks to the European portion. The object of the expedition was to put down the notorious Dacoit chief, Mow-gowng-gee. The expedition marched from Prome, in a south-easterly direction, to a place called Taphoon, about 60 miles distant, where they arrived on the 5th day, December 30th, having marched from 8 to 17 miles a day. Though the march was commenced before daylight every morning, yet, from the badness of the roads, and the real or feigned ignorance of the native guides, who often led the force by most circuitous routes, the new encamping ground was seldom reached before noon or even later, the men being thus exposed for several hours to a burning sun, and many of them not arriving in camp till late in the afternoon, perfectly exhausted. At Taphoon the force established its head-quarters from the 30th of December to the 1st of February 1854, the tents being pitched in an extensive rice-field, surrounded by trees and dense jungle. From this centre, numerous detachments were sent out in different directions into the surrounding country, in search of the dacoits. The men on these expeditions sometimes remained out for several days, often without tents, were subjected to great fatigue, and had to march sometimes under a burning mid-day sun, and sometimes through the chilly fogs of night and morning. The provisions supplied to the force were necessarily very inferior to what the men had been accustomed to in cantonments. They had no bread, butter, milk, and but few vegetables, and subsisted almost entirely on tough beef and biscuit, while the tents in which they dwelt afforded them but little protection from the great heat during the day, and the cold and damp of the morning, The men, too, became dispirited, from their several expeditions being seldom attended with success. The force left Taphoon on the 1st of February, and arrived at Prome again on the 10th. For the first few days after the force left Prome there were no admissions into the hospital of the 2d Europeans, except a few cases of extreme debility from fatigue, but Table VIII. shows the great number of admissions into hospital, and the comparatively great mortality, from the 1st of January up to the 10th of February.

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[1] We here estimate the number of sick in Nos. 1, 2, 3, and 5 companies at 12 in each, for the reasons above specified.

Table VIII.

Strength, 250 Fighting Men.	Diseases.											Total.	Per Centage.
	Apoplexy.	Colic.	Constipatio.	Debilitas.	Diarrhœa.	Dysentery.	Febris Int. Quot.	Do. do. Tert.	Do. Remittens.	Pneumonia.	Tænia Solium.		
Admitted,	1	1	1	2	12	3	71	8	7	1	1	108	43·2
Discharged,	„	1	1	2	8	„	64	6	4	„	1	87	
Died,	1	„	„	„	„	1	„	„	1	1	„	4	1·6
Transferred to Hospital at Prome,		„	„	„	4	2	7	2	2	„	„	17	

Thus, in the space of six weeks 43·2 per cent. of the whole detachment had to be admitted into hospital, and 1·6 per cent. died. It may be also observed that a number of the less severe cases of Intermittent fever were treated as out-patients, and are not included in the above Table.

Again, out of the 40 men and 2 officers of the Madras Horse Artillery, both the officers had fever, and 30 men, or 75 per cent., were admitted into hospital, besides several others treated as out-patients. The diseases were similar to those above-mentioned. One of the men died of remittent fever.

The detachments of the native regiments from Bengal and Madras suffered equally with the Europeans.

Lastly, it was observed with regard to the various detachments which composed this expedition, that for many weeks after their return to Prome the number admitted into hospital was far greater from among them, than from other portions of the same regiments.

From the previous Tables (V., VIII.), it is evident that the principal diseases which prevailed among the troops were fever, dysentery, and diarrhœa, but more particularly the first of these. Thus, in Table V., out of the 435 men admitted into hospital, 328 were on account of various forms of fever. Of these 328,

4 were continued fever.
105 were remittent fever.
And 219 were intermittent fever.

Again, in Table VIII., out of the 180 men admitted into hospital, 86 were on account of fever, 79 intermittent, and 7 remittent; and of the 30 men admitted from the Madras Horse Artillery, 24 were fever cases.

Now, it is interesting to contrast with these results, the cases of fever which occurred in the same regiment at Calcutta in the hot dry months immediately preceding the rains. During the months of May and June 1853, out of a strength of about 600, there were admitted into the regimental hospital at Calcutta, 209 cases of fever. Of these,

173 were continued fever.
2 were remittent fever.
And 34 intermittent fever.

It is to be observed, that during the hot dry months of May and June at Calcutta, there were 173 cases of continued fever, and only two of remittent, while, in the months of November, December, and January, after the rains at Prome there were 105 cases of remittent fever, and only four of continued, and even these four cases differed remarkably from the continued fever of the hot dry season at Calcutta, and might more properly be classified along with the co-existing cases of remittent fever, the only difference being, that there were no well-marked periods of remission. The continued fever at Calcutta was sthenic, generally demanding copious depletion, while the symptoms of the remittent fever at Prome, were for the most part adynamic, with a tendency to become typhoid, not bearing depletion, but, on the contrary, requiring the early use of stimulants.

Again, cases of intermittent fever were more numerous, after the rains at Prome, than during the hot dry months preceding these at Calcutta. Thus during the months of May and June at Calcutta, only 34 cases of intermittent fever were admitted into hospital, while during the months of November and December, at Prome, there were 154 cases, the strength being nearly the same in both cases.

These differences in the form, type, &c., of the fever at the two periods, may be partly accounted for, by the difference in the season of the year, but there can be little doubt that the climate of Prome, and the other pre-disposing and debilitating causes about to be enumerated, had much to do with the adynamic and typhoid tendency, which the fevers, and indeed diseases, there exhibited. But this leads us to consider e fourth place :

. *The Causes* to which the diseases just enumerated e attributed. From a perusal of the tables above given, it must be at once obvious, that malaria were the proximate exciting causes of disease in the majority of cases, but there were also many other circumstances, which may have served as causes of predisposition, and determining exciting causes, and which it will be useful for us to consider. We shall therefore in the first place, say a few words on the subject of the malaria themselves, and afterwards allude to the other accessory causes of disease.

The nature and origin of malaria or marsh miasmata are still enveloped in mystery, and, indeed, it still remains to be shown that they have a substantial existence. Expert chemists have repeatedly analysed the air of malarious districts, but have never succeeded in demonstrating the presence of any poisonous principle. The general impression, however, is that such a principle exists in the atmosphere in the form of a gaseous fluid. Again, as to the origin of malaria, though the general belief among medical men has been, that they are produced by animal, and more especially vegetable matter, undergoing decomposition, yet the observations of Dr William Ferguson[1] have led many to believe, that malaria and the products of vegetable decomposition are two distinct things, and that though they may co-exist, they have no necessary connection. We cannot help thinking however, that the deductions which have been drawn by Dr Ferguson and others, have been somewhat too hasty. The fact stated by Dr Watson,[2] that we may, in this country, often have putrefying vegetable matter, without any malarious diseases, is merely a negative argument, and one of little value, for other concomitant circumstances necessary for their production, may in such cases be wanting. Again, though Dr Ferguson has brought forward numerous very interesting instances of malarious diseases being very prevalent in localities, where there was said to be no vegetable matter, either putrefying, or to putrefy, yet we believe, that if we may except the sandy deserts of Asia and Africa, careful examination would shew, that few parts of the earth's surface are entirely destitute of vegetable remains, if not visible to the naked eye, at all events of a microscopic nature. We have repeatedly submitted to microscopic examination the water from the swamps of Burmah, and have found it to Abound in Confervae, Desmeidiæ, Diatomaceæ, and Infusorial animalcules, and we believe that there are are few parts of the world where these are absent. The decomposition of these microscopic beings might suffice to generate malaria, for we know by experiment that air contaminated with so little as $\frac{1}{36000}$ part of hydrochloric acid gas will destroy the life of plants,[1] and the poisonous principle of malaria must exist in even smaller quantity, for while we can detect the presence of $\frac{1}{36000}$ of hydrochloric acid gas in the atmosphere, that of the poisonous principle of malaria we are unable to demonstrate. The probable existence of such beings was perhaps lost sight of by Dr Ferguson in his observations. But, be the origin and nature of malaria what they may, there can be no question, that at Prome, at the period we have been considering, those circumstances, which in most cases, coexist with the production and noxious effects of malaria existed in a high degree. A few of these may be mentioned.

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[1] "On the Nature and History of the Marsh Poison." *Edin. Phil. Trans.* 1821.

[2] Lectures on the Practice of Physic, vol. i., p. 733.

1. The *season* in which the troops reached Prome, was that during which malarious diseases in the tropics are known to be most prevalent, viz.: that immediately succeeding the cessation of the rains, or, as it is called the "drying up" of the rains.

2. The country surrounding Prome abounded in *dense jungle,* and even the sides of the hills, on the summit of which the barracks were built, were covered with copious vegetation. Shortly after the arrival of the troops at Prome, the sides of these hills were cleared, and the brushwood, after drying for some days in the sun, collected and burnt. The propriety of such a step, at this time, seems very questionable, for it has always been found in malarious countries, that clearing the soil, and thereby exposing the earth's surface to the more direct action of the sun's rays, has increased disease, and rendered its character more severe. It may be mentioned too, that disease did not shew itself to any great extent among the troops, until after this clearing had commenced, but of course it would be difficult to say how far this was instrumental in producing it.

3. The country surrounding Prome also abounded in *low swamps* and extensive *rice-fields.* These swamps and rice-fields are liable to great inundations during the rainy season; so much so, that in many places the principal communication between the different villages is carried on by means of boats. In the protracted dryness after the rains, these rice-fields become split up by wide fissures through the soil, often two or three feet in depth. Through these fissures the vapour passes up from the lower portions of the soil; and it has been observed, that the vapours which escape from such fissures are often more noxious than the emanations from a wet marshy ground.[2] In connection with this, it may be observed,

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[1] Traill's Medical Jurisprudence, p. 124.

[2] Copland's Medical Dictionary, vol. i. p. 758.

that one of the prevailing winds at Prome blew in a direction from some of these rice-fields towards the barracks. Often in the early morning we have observed a cloud of mist wafted along the earth's surface for miles; and it has been thought that malaria, whatever be their nature, cling to such mists.

4. *The site of the barracks* on the summits of the different hills was such as to expose them to any malarious poison wafted by the winds. Dr Copland remarks, "high grounds, near exposed marshes, are often more unhealthy than the places immediately adjoining, that are on a level with them." We may mention, too, that none of the native Burmese will live on these hills, which are regarded by them as most unhealthy localities.

5. *A high temperature* has always been found favourable to the production of malaria; and we have already endeavoured to show, that the temperature at Prome was greater than in most parts of India at the corresponding season.

6. The whole of Burmah swarms with myriads of *insects* and *reptiles*, even to a greater extent than most parts of India. How far the decay of their remains may tend to the production of malaria, we know not; but we have thought the circumstance worthy of observation, as Dr Copland, in speaking of malaria, makes the following statement:—"I have always considered the number of insects and reptiles, with which a place abounds, as more indicative of its insalubrity, than almost any other circumstance." Again, he observes, that a mixture of animal and vegetable matter undergoing decay gives rise to miasms, "much more noxious than those resulting from vegetable matter alone."[1]

Under such circumstances as the above, we are not surprised at the generation of malaria. But, before we leave this subject, one word on the so-called *varieties* of the malarious poison. Is the poison which gives rise to a quotidian ague different from that which produces a tertian; or this different from that which produces a remittent fever; or are all these different from that which gives rise to dysentery—or, again, is there only one poison whose effects will vary according to its intensity, and the constitution and predispositions of the individuals exposed? These are questions of great importance, but of very difficult solution. When, however, we see a large body of individuals placed under the same circumstances, and exposed to the same causes of disease, some of whom are seized with quotidian ague, others with tertian, and others with remittent fever, the presumption is that the same cause has produced different diseases, according to its intensity, the constitution of the individual and the predisposing causes to which he may be subjected. Again, as favouring this presumption, we see remittent fevers passing into quotidian, and quotidian into tertian; and we have observed in Burmah, that persons who have had repeated attacks of fever are liable to be attacked by dysentery. The fact already alluded to, that the form and severity of malarious diseases vary with the season of the year, would seem to indicate that these depend on the intensity of the malaria; the malaria, for instance, generated after the cessation of the rains being more intense than at other periods. Again, the constitutional predisposition of the individual would seem to influence the form of the disease produced, under such circumstances as the following, which happened to a medical friend, when surgeon of a vessel anchored in the Hooghly:—A party of the crew slept for a single night on the swampy banks of the river; within a few days almost every one of the party was laid up: some with ague, some with remittent fever, and others with dysentery.

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[1] Copland's Medical Dictionary, vol. i., p. 758.

Let us now proceed to consider a few of the predisposing causes of disease among the troops In considering the predisposing causes of a disease, we must distinguish between the constitutional predisposition of the patient himself, and the causes which may lead to predisposition. Thus, we find, among a body of men, all exposed to the same causes of disease, both predisposing and exciting, that some are far more prone to disease than others. Nothing could have illustrated this better than some of the fever cases at Prome. Thus for instance, one man was admitted into hospital during the months of December, January, and February, no fewer than nine times with intermittent fever, and the same patient stated, that, ten years before, he had been admitted into hospital at Meerut, seven times in three months, for the same complaint. Here, then, we have a remarkable predisposition to fever, which can only be explained by some peculiarity in the patient's constitution. Such a constitutional predisposition is well known to be induced by previous attacks of ague, and most of the men had suffered from ague during their service in India. A few of the men, however, who had been in India for nine or ten years without ever having had an attack of ague, were seized at Prome for the first time almost immediately after their arrival.

Among the causes tending to produce predisposition to disease, to which the men were exposed, may be mentioned the following:—

1. *Immigration* to a new country, even in the same latitude, has always been found to predispose to disease.

2. *Fatigue* debilitates the system, and thereby renders it more liable to be influenced by the exciting causes of disease.

3. *Travelling*, independent of great fatigue, has often been observed in India to predispose to disease, and we may expect that it will do so still more, when accompanied by fatigue, exposure to the inclemencies of the weather, and various privations, such as those to which we have found the troops we have been considering to have been exposed. The truth of this remark was forcibly illustrated by the marked difference in the amount of disease, in the three different detachments of the regiment already alluded to, and also by the great prevalence of sickness in the detachment, which was sent out into the district.

4. *Sleeplessness.*—We have already alluded to the fact, that the portion of the regiment which suffered most from disease, after its arrival at Prome, had scarcely slept any during their passage from Rangoon on account of the mosquitoes. Now we know, that want of sleep gives rise to a state of depression of the system greatly predisposing to disease.

5. *Depression of spirits* is well-known to be a powerful predisposing cause of disease, while cheerfulness is one of the best protecting influences against it. European troops in India, with nothing to occupy their minds, can seldom be said to be very cheerful; and this remark applied with double force to Burmah. In fact, there has been such an antipathy amongst our troops to going to Burmah, that *Burmahphobia* may be reckoned as having been a very strong predisposing cause of disease.

6. *Bad food.*—As might be expected, the troops on going down to Burmah had few of the comparative luxuries to which they had previously been accustomed in India, and this was particularly the case on their first arrival, and in the detachment sent out into the district.

7. *Bad water.*—We think we can scarcely include this among the causes of disease. The water of the Irrawaddy, which the men drank, though very muddy, yet after filtration through sand and charcoal, we found to be remarkably clear, and free from chemical impurities, containing only slight traces of chlorides, and carbonate and sulphate of lime. It may be mentioned, however, that the detachment sent into the district had to drink the unfiltered water from the native wells, which in many cases was thick and muddy.

8. *Intemperance.*—A small allowance of stimulants was no doubt useful under the circumstances in which the troops were placed; and the substitution of ale and porter for

spirits has always been found beneficial; but many of the men were intemperate, and thereby disposed their systems to the influence of the malaria.

9. *Night duty.*—Exposure to the night air in malarious districts has always been found very dangerous. The malárious poison appears in the night time to be in a state of concentration, while, at the same time, the system is more susceptible of its influence. Many of the men were first seized with fever when on sentry duty at night. We may here allude to the supposed injurious effects of sleeping in the light of the moon. It is a very general belief among old and experienced officers in India, that sleeping in the light of the moon is highly dangerous, producing paralysis of the face, fever, and other diseases. Many of these officers will bring forward cases, which they believe to be perfectly conclusive; but, among the many with whom we have conversed on this subject, we have found none who could bring forward a case in which the injurious effects did not admit of a more ready explanation from other concomitant circumstances, such as the evaporation of the night dew, malaria, &c. We are therefore inclined to disbelieve the baneful influences which have been attributed to the lunar rays.

We shall now mention one or two of what may be called the determining exciting causes of the diseases which prevailed. There is every probability, that a contagious or malarious poison may exist in the system for some time, without manifesting itself, until some cause is brought into play which determines its manifestation. Of this nature the following may be enumerated as having been in operation at Prome:—

1. *Chills.*—These are well-known to be powerful exciting causes of many diseases, and in the case of malarious diseases they are no doubt as powerful determining exciting causes. We have seen that during the months of November and December the mornings at Prome were peculiarly chilly, and we may mention that most of the fever patients, who could give any cause for their illness, attributed it to cold, caught during a night march, or from throwing off their jacket on returning from parade in the morning, or from exposure to a cold draft during the night. In connexion with this, too, it may be observed that the walls of the men's barracks consisted only of thin matting, allowing currents of air to pass freely through, and affording but little protection from the cold.

2. *Exposure to the direct rays of the sun.*—We have already called attention to the great daily alternations of

temperature at Prome. Even when the morning was very chilly, the temperature during the day was high, and on no day was exposure to the sun's rays at all pleasant. The most of the men who were necessarily exposed, suffered from cephalalgia, and other symptoms of cerebral congestion, and many, a few hours after, were attacked by fever.

3. *Intemperance.*—Habitual intemperance has been already alluded to as a predisposing cause of disease. There can be no doubt also that individual acts of intemperance may act as determining exciting causes. On their first arrival at Prome, cases of intemperance among the men were not very frequent; but, after a few weeks, ennui drove them to the bottle, and cases of drunkenness were not rare. The liquors, too, which they had often recourse to, were most pernicious, being the native toddy, or fermented juice of the Tari palm, and Sham-shu, a very intoxicating liquid, obtained by the Chinese by the distillation of rice.

The great influence of these predisposing and determining exciting causes, is well shown by the remarkable exemption from fever of all the officers, on whom these causes operated in a minor degree. During the first three months there were only two officers attacked by fever, and these were attacked under the following circumstances. On Sunday, November 27th, these two rode down to Shoaydong, eight miles distant, had a late dinner there, and at 11 P.M. started for Prome again by the river, in an open native boat. They were three or four hours rowing up against the current, and having neglected to take their cloaks with them, arrived at Prome, drenched with dew, and very cold. On the Tuesday following they were *both* attacked by intermittent fever, which lasted for some days. These cases are also interesting as regards the latent period of intermittent fever. In the agues of this country we know that the latent period sometimes extends over nine months, but in the tropics it would appear in general to be much shorter. In the above cases the latent period was less than two days; though of course, it might be argued, that this had commenced previous to the 27th, and that the exposure to the night air only acted as determining exciting causes; but, that this should have happened in both cases is not probable.

Again, the influence of these two classes of causes is shown by the remarkable prevalence of disease, where many of the causes were in operation at one and the same time, as, for instance, in the case of the first detachment of the regiment which arrived at Prome, and in that of the detachment sent into the district.

(*To be continued.*)

ART. VI.—*On the Facts and Opinions recently recorded in regard to Asiatic Cholera.* By J. WARBURTON BEGBIE, M.D., Fellow of the Royal College of Physicians, lately Physician to the Cholera Hospital, Surgeon Square, Edinburgh.

(*Continued from page* 523 *of former Volume.*)

No point in favour of the non-contagious property of cholera has been referred to more frequently, nor, unquestionably, in many instances, with a greater show of reason, than the exemption from the disease enjoyed by the attendants upon the sick, both medical men and nurses. Let us, in the first place, glance at some of the facts relating to this subject, which have been adduced. " The late Dr Reid of St Andrews," writes Dr Alison,[1] " whose accuracy of observation is well known, and who went from Edinburgh to Dumfries and took charge of one of the districts of that town, at the time when it suffered more than any other town in Scotland from the epidemic cholera in 1832, expressed himself distinctly as satisfied, as all the medical men in Dumfries at that time were, that those who had close intercoursé with the sick at that time, in that town, were not affected with the disease in a larger proportion than those who avoided such intercourse; and Dr Blacklock of Dumfries, who saw much of the disease when it prevailed again pretty extensively there during the winter of 1848, found that the same remark might be made upon it at that time." Among other practitioners in Dumfries, who, during the epidemic of 1832, saw much of the disease, was Dr Charles Bell, now of this city; and he, too, willingly subscribes to the view taken by Dr Reid. Indeed, it would appear that the non-contagious view of cholera has been embraced by nearly all the medical men who treated the disease in Dumfries.[2] Dr Grieve has written a very interesting report on cholera, as it prevailed in Dumfries in 1848.[3] " In regard to the much-vexed question, is cholera contagious or not?" he says " I have carefully and without any preconceived opinions to support, examined and dispassionately weighed all the facts connected with the history of both epidemics, and I am bound to say, that I have no rational grounds for believing it to be contagious; on the contrary, I believe that it has its origin in, and is propagated by, other sources than the communication of the sick with the healthy, and that exhalations from the human body have not the power of reproducing the disease." Dr Grieve then goes on to prove that cholera, as it prevailed in Dumfries in 1832 and 1848, had a spontaneous origin, and that no sources of contagion or infection existed; adding what concerns us most at present, " nor have I found in my experience of both epidemics, that those more particularly exposed to the influence of contagion by attendance on the sick, viz., medical men, nurses, and relatives, were more liable to be attacked with the disease than others not so exposed. Not one of our nurses in Dumfries was seized, though engaged night and day, in all possible conditions of moral and physical depression, in attendance upon the sick. Upwards of eighty cases of cholera were treated in the Infirmary; and when the first case of cholera was admitted, there were about thirty patients in the house, none of whom took the disease." These statements of Dr Grieve are sufficiently startling and important. They are not, however, singular in regard to the facts they communicate. Dr Begbie, who during six months had charge of the Castle Hill Cholera Hospital in 1832, has stated " that not one medical-officer, student or nurse, took the disease.[1] During the panic, moreover, many persons found lying in the streets were sent to the Cholera Hospital, put into warm beds formerly occupied by cholera patients, and thus most fully exposed to the influence of contagion; and yet, in no instance, had he known these people to be affected with cholera." In regard to this point, we find Dr Baly writing (page 180) " the evidence in several instances specially reported on is apparently most decided as to the safety of nurses attending on patients under such circumstances. Thus, it results from the evidence of Dr Kidd and Mr Allen of Oxford, that the nurses at the Cholera Hospital in that city, although they suffered from diarrhœa, were in no case attacked with cholera; and Dr Hawkins and Dr Basham testify, that at the Middlesex and Westminster Hospitals, into which many cholera patients were admitted—36 dying in the former, and 37 in the latter—no nurse, pupil, or medical-officer, was attacked. In the London Hospital, where 40 patients admitted for attacks of cholera died, no case of the disease arose in the Hospital among the ordinary patients, or among the nurses even of the cholera wards. In St George's Hospital, again, according to Dr Barclay, the medical registrar, there was no case among the nurses or persons who were brought into contact with patients suffering under the disease; and 'none in those wards in which cholera patients had at any time been placed.' Of five cases that occurred in this hospital, one was a nurse 'whose duties kept her entirely out of the way of cholera patients,' and two of the others were surgical patients. Lastly, Dr Parkes states, that at University College Hospital, although many patients suffering from cholera were admitted, and 31 died, not one of the sick nurses who attended on them was attacked; and the single patient admitted for another disease, but seized with cholera in the hospital, though in an adjoining ward, was not known to have had direct communication with the cholera patients." It is here scarcely necessary to remind our readers of the facts of a totally contrary bearing formerly adduced—for while it is very generally admitted that the former hold more frequently true than the latter, this, too, is quite certain, that the experience of hospitals in this respect very greatly differs. "The communications received by the Cholera Committee," writes Dr Baly, "afford, indeed, examples of the most contradictory character." Some of the instances which have been detailed—though not fully, in the Report of the Cholera Committee—entitle Dr Baly to suggest the explanation, that the nurses and other attendants suffered from cholera, not in consequence of communication with the sick, but owing to the Hospitals themselves having become foci of the disease. In our own opinion, however, as well as in Dr Baly's, this view cannot explain the communication of the disease to the attendants, in some of the instances quoted in the former part of this paper, and many others given in the Report of the Cholera Committee. Since the end of August last the Cholera Hospital in Surgeon Square, which was closed during several months, has been again open, and nearly two hundred cases of the disease have been treated, the greatest number of patients in hospital at any one time being nearly thirty (on the 21st, 22d, and 23d of September). During this period there has been no instance of the communication of the disease to any attendant; while the assiduous and unwearied attentions of Dr Asher and Messrs Falconer, Clark, and Wilson, the House physicians, and of all the nurses employed, have, at all events, once more fairly tested the contagious power of Cholera in that Hospital. As regards Hospitals, then, it appears undoubted, that while in some there were instances of the communication of the disease to the attendants, in others no such occurrence took place. Do we possess any means of accounting for this discrepancy as regards the individual Hospitals?

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[1] British and Foreign Medico Chirurgical Review, Jan. 1854.

[2] Dr Barker, physician to the Dumfries and Galloway Infirmary, does not adopt this view. He says, " I am by no means sure that cholera did not spread by contagion."—*Monthly Journal,* 1849.

[3] Monthly Journal, May 1849.

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[1] Discussion in Medico-Chirurgical Society, December 7th 1853.

If there existed any peculiar circumstances in the situation and accommodation of one or more of these, as rendering the attendants connected with them more prone to the contagion of cholera, of this we are quite certain that these circumstances were exceptional, and that now we are called upon to determine this point, irrespective altogether of the Hospitals individually.[1] No more can "the different states of susceptibility of different persons to the disease," account for the discrepancies in Hospital experience, for while that circumstance, as Dr Baly allows, may have had a share in determining whether the nurses became affected or not, it cannot altogether explain the facts. As regards the attendants upon cholera patients not in Hospital, and their exemption from contagion, the facts adduced are, as yet, neither sufficiently numerous nor exact, to render their examination of much use in this inquiry; it will, however, we think, be admitted by all, that the known proportion of those medical men and nurses who have fallen victims under such circumstances is very small indeed, as compared with those who have acquired fever or any other contagious disease, and that such cases as have occurred have not unjustly been attributed to some peculiarity in the case of the infected person, of which over-exertion, fatigue, mental and bodily, and the exposure during a long period to a polluted atmosphere, have been the causes generally assigned. We cannot avoid the reflection that, in all the cases known to ourselves, in which medical men have been attacked with cholera, one or other, or all of these calamitous influences, have been at work.

In reviewing all that has been advanced upon this subject (of which the facts we have here been able to allude to must be taken as examples merely, very many others of like interest and importance being given in the Reports on the Progress of Cholera in 1848–49 in the Monthly Journal, and in Dr Baly's Inquiry), the belief, that cholera is a disease capable of communication by contagion surely gains ground. No doubt the testimony of some thoroughly competent and experienced physicians is opposed to it, still there remain so many instances of the disease attacking those brought more immediately in contact with it, if not in greater numbers, at all events, in a manner so striking as to convince us that, in these instances, the sick were the means of communicating the disease. This is all, however, that a very careful examination of the facts permits us to consider as determined.

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[1] If any one circumstance can be considered as setting this point at rest, surely it is seen in the fact of the medical attendants and nurses *not* suffering in greater numbers, in those Hospitals which are exclusively devoted to the reception of cholera patients, or in those wards in general Hospitals into which cholera victims are alone admitted; another circumstance of similar import is found in the experience of the same Hospitals at different periods, and during different epidemics. Take, for example, the Cholera Hospital, Surgeon Square, Edinburgh. We have already referred to Dr Robertson's report in 1848; five nurses had cholera and three died. In 1853, a female connected with the laundry took the disease and died, and in 1854, when the number of patients in Hospital was much greater than in the former year, and the strength and devotion of all the attendants much more severely tried, *no case* originated in the Hospital.

Before passing from this branch of our inquiry to the consideration of the more general question of the diffusion and mode of propagation of cholera, we must refer to another matter intimately bearing upon the one we have just considered, viz.:—The communication of the disease to the lower animals. Upon this subject experiments with cholera blood, with the ejections and dejections of cholera patients, and exposure to the exhalations of cholera victims, have been made; for a very able and interesting account of all that had been done, up to the period at which he wrote, as well as of his own experiments, we must refer the reader to an article in the British and Foreign Medico-Chirurgical Review for 1853, by Mr Marshall of University College Hospital. This author concludes by stating "that though short of actual proof, the evidence, both general and particular, accidental and experimental, is not unfavourable to the notion of a certain susceptibility of the dog, and perhaps too of the cat, to the influence of the cholera-agent, whatever that may be." Still more recently Dr Lauder Lindsay has made public, through the pages of this journal, a series of elaborate experiments, not only with the evacuations, but with fomites; the performance of the latter experiment had occurred to Mr Marshall in 1849, "but whilst gradually advancing in his inquiry, the epidemic happily ceased in London." Here it is, of course, unnecessary to detail the steps of Dr Lindsay's very important and minutely performed experiment with the fomites; the result of it was eminently successful, and to this gentleman belongs the merit of having first exhibited the communicability of cholera to the lower animals, and so *determined* the contagious nature of cholera.[1]

We have then arrived at the fact that cholera is a contagious disease, and we have also freely admitted, and shall presently admit more fully, that contagion will not explain the occurrence of numerous cases and outbreaks of cholera any more that it will explain the exemption of those from the disease who have close intercourse with the sick. It may be inconsistent to say that a disease is both contagious and non-contagious, but as Dr Alison has observed "there is no inconsistency in saying, that the poison exciting a disease may be both propagated by contagion (*i.e.*), by intercourse of the healthy with the sick, and likewise may, either through the atmosphere, or in some other way be diffused, to a certain extent, over the surface of the earth, independently of any such close intercourse." Cholera is undoubtedly propagated without the means of human intercourse—the contemplation of its march and progress in its grand epidemics, as well as of its more isolated outbreaks, has forced this consideration upon all, and caused us to seek for some other agent or agencies whereby its diffusion is accomplished. In some instances, it would appear as if some *limited*, though energetic cause alone operated, in which the disease is confined to a particular district or districts, and its extension is manifestly unconnected with the intercourse of the sick with the healthy. Of this Mr Hamilton Bell, in his well-known work, gives the following among other exemplifications, in the case of Lord Hasting's army of nearly 100,000, (quoted by Dr Alison):[1]—"When the disease was at its height, and all business had given way to solicitude for the suffering, the army was moved on the 13th November 1817, and after marching above forty miles, and reaching the high and dry banks of the Battah, halted on the 19th. The road, it is stated, was strewed with the dead and the dying, and the ground of encampment presented the appearance of a field of battle. But here the army got rid of the pestilence which ceased to be epidemic on the 22d; so that this crowded camp, carrying thousands of sick along with it, by moving forty miles, in less than ten days *shook off the disease*." Many other examples of a similar nature might be adduced, in which the only explanation we are as yet able to offer is, that certainly independently of human intercourse,—the poison of cholera, in some ill-understood manner, operates from some limited telluric or atmospheric cause. That the atmosphere plays an important part in the propagation of the disease does not admit of doubt. "At Madras," writes Dr Parkes,[2] "the disease is heard of at a station ninety miles off; a few days afterwards, it appears in Madras itself." In such cases, besides the certainty of human intercourse not having occurred—which in some instances is not absolute—the rapidity of the transmission of the disease is in itself sufficient to negative the agency of contagion. Atmospheric currents, whose progress are sufficiently rapid, are then in all probability the medium of its diffusion; and we cannot resist the conclusion that, in those instances where the outbreak of the disease has been sudden, and where several hundreds or thousands are known to have been attacked by cholera within a few days or weeks, its transmission can only be accounted for through the agency of atmospheric currents. The following facts, observed at Madras, are given by Mr Scot (Report on Epidemic Cholera in the Presidency of Fort St George), in support of the view that the infectious principle of cholera may be conveyed by the air. "His Majesty's 54th Regiment landed at Madras on the 10th of May from the H. C. ships *William Fairlie* and *Thomas Coutts*, in a remarkably healthy state, after a voyage of 48 days from the Cape of Good Hope, and marched into quarters at Fort George. Cholera appeared amongst the men within three days after their landing. In the former ship, while at anchor in the roads, cholera made its appearance on or about the 18th of May; they had sixty-five cases, and of these twelve proved fatal. The disease did not appear in the *Coutts* till a fortnight afterwards. They had only twenty-three cases, of which six were fatal. Several circumstances connected with the appearance of the disease on board these ships deserve to be noticed. No case of cholera occurred in either ship while out at sea. The *Fairlie* lay at anchor so far to the southward as to be directly to leeward of the fort, and this was during the prevalence of the strong south-west winds. The disease was at that period established in the 54th Regiment in the garrison there. The men who worked upon deck, and those who slept on the landward side of the ship, were found to be decidedly the most obnoxious to the attacks of cholera. The ship afterwards took up a position farther north, and about a quarter of a mile directly to windward of the *Coutts;* although cholera had then existed in the former ship for nearly a fortnight, one solitary case only had as yet appeared on board the latter ship. This case was in a sailor who slept on deck in a state of intoxication during the night. Thus situated, the wind blew directly over the *Fairlie* to the *Coutts*, and the disease at this juncture appeared in the *Coutts;* all the men attacked, with one exception, slept on the side of the ship next to the *Fairlie;* this side, however, was likewise the landward side."—(Dr Baly's Report, p. 190). Many other instances of a similar nature might be added; and in both Bengal and Bombay have the experiences of ships been the same. Our readers, who, accepting the agency of atmospheric currents in carrying the poison, reject the evidence in favour of the contagious property of cholera, would find a difficulty in accounting for what has undoubtedly frequently occurred, viz., the disease travelling in the opposite direction to the wind, and indeed "in the teeth of the monsoon." In this country, Dr Brown of Sunderland has remarked, that the house in which the first case on shore occurred was situated at some distance, nearly a mile from where the infected ships lay, but exactly in the direction in which the prevailing easterly winds would convey any effluvia from the vessels, without interjacent obstacles to its reaching the town."—(*Cholera Report*, page 192). Other observations of a like kind have been made in England and Ireland during the epidemic of 1848–49. "The belief," says Dr Baly, "that the cholera poison is wafted from place to place by the wind, rests almost entirely on the ground of inference, although it is an inference scarcely admitting of question, if the theory which regards the cause of cholera as a poisonous matter existing and increasing in the air, but not diffused through it in the manner of gas, be the correct one." Various theories have been promulgated with the view of accounting for the manner in which the morbific matter of cholera acts through the atmosphere, or, at all events independent of human intercourse. One of these ascribes it to swarms of microscopic animalculæ or vegetative germs which are taken into the bodies of persons exposed to them, multiply there, and excite the disease, but are capable of diffusing themselves through the air and of attaching themselves to, and *rapidly multiplying* on, other matters likewise." Again, Dr Snow has propounded a theory which appears to have gained some acceptance; he makes the solid and liquid articles of food, instead of the atmosphere, the medium by which the morbific matter is introduced into the system. "He supposes that the poison being swallowed, acts directly on the mucous membrane of the intestines, and excites the flux from its surface; that the poisonous matter is at the same time reproduced in the intestinal canal, and passes out much increased in the discharges; and, lastly, that it then in various ways, but chiefly by those discharges becoming mixed with the drinking-water in rivers or wells, reaches the alimentary canals of other persons, in whom the like disease and accompanying reproduction of the poison ensue." This theory received at the first some show of probability from the experiments and observations of Dr Britain and Dr Swayne, who found the matters which had passed through the alimentary canal to exist in some kinds of water used for drinking, and for culinary purposes." More recent observations, however, have completely exploded this ingenious theory, in so far as it rests upon the supposed presence of peculiar matter in the evacuations. "I have seen," says Dr Lauder Lindsay, " all the forms of the so-called annular bodies" described by Britain and Swayne, in the evacuations of patients under my own charge, and I have also seen the same fluids abounding in minute vibrins in a high state of activity; but having observed the former only in small quantity, and being able in many instances to trace them back to their original source, the food; believing, moreover, that the latter exist equally in all decomposing animal fluids, I could not look upon either as having any casual relation to the disease, and therefore did not anticipate any specific effects from the administration to animals of fluids, which I knew by microscopical examination to contain both in abundance." The theory of the cause of cholera being distributed with the drinking water, is very carefully investigated by Dr Baly in his report, and as a whole "he pronounced it" untenable. " It has however, directed attention to circumstances which may be hereafter shown to bear a part in the production of this as well as other epidemics." In concluding this part of our inquiry, having, already acknowledged that there exist many circumstances in regard to the diffusion and mode of propagation of cholera which are only explicable on the admission that human intercourse plays the most important part; and having also acknowledged that human intercourse does not, and cannot explain the propagation and diffusion of the disease in certain and very many other circumstances; we feel, while allowing that many most important points upon this subject will in all probability be hereafter disclosed—that in some manner or other but ill understood, cholera has the power of diffusing itself, " whether through the atmosphere, or along the earth's surface, or under the earth." "Why not admit" says Dr Alison, " that the disease can extend itself in two distinct ways; one simple and easily understood, the other more obscured, but equally ascertained and probably even more destructive."

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[1] The result of Dr Lindsay's experiment with the fomites taken in connection with the now acknowledged communication of the disease to washerwomen especially, and others who have handled the clothes of deceased friends, &c., appears to us to start a most important line of further inquiry, and to lead to the conclusion that the influence of the morbific agent in cholera is first exerted through the respiratory system.

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[1] British and Foreign Medico-Chirurgical Review, January 1854.

[2] Baly's Report, p. 191.

Morbid Anatomy and Pathology of Cholera.—The appearances found on dissection of the bodies of those who have died of cholera vary considerably in respect to the period or stage of the disease at which death has taken place, whether in collapse, or after reaction has occurred. We shall attend to these in their order, and first of all inquire what is the condition of the body after death has occurred in collapse. During the epidemics of 1832 and 1848, as well as during the late epidemic, it was noticed by very many observers, that a rise of temperature, and certain very remarkable muscular contractions, occurred shortly after death. Concerning the former phenomenon, Dr Gull has referred to cases in which, by careful measurements, it was ascertained that a rise of temperature had really taken place. "In case 2, during life, the coldness of the surface was very marked; two hours and a half after death, the thermometer indicated 102·12°. In case 6, ten minutes before death, the temperature in the axilla was 103·1°; five minutes after death, it was 104°. In case 9, twenty-five minutes after death, the temperature of the uncovered abdomen was 89°; forty-five minutes after death, it was 90°; and fifty minutes after death, it was 91°." In some cases, where a decided increase of temperature after death has not been observed, the body has retained the warmth which existed at death for a remarkable period of time. "In case 1," says Dr Gull, "five hours before death, the temperature was 95° in the axilla; the patient remained in the same collapsed state, and died half an hour after mid-day; three hours and a half after death, the temperature in the axilla was 95·36°, and in the groin, 97·16°, being an increase of nearly half a degree above that noted during life. In case 2, the temperature was 101·12° two hours and a half after death. In case 9, the surface of the abdomen retained its temperature unaltered for forty-five minutes." In case 10, the heat observed at the moment of death was not only maintained, but an hour afterwards was nearly 2° higher."

The muscular contractions, which, for the most part, are confined to the extremities, but have also been frequently observed to affect the jaws, neck, and face, and also the trunk, begin a short time after death,—according to our own observation—from five minutes to forty minutes, and last a variable period. Under the influence of galvanic stimulus, the period of contraction could be kept up for hours, but without this aid, the movements, as a general rule, ceased after a few minutes, or, at most, one hour had elapsed. The late Mr Barlow, who, more than any other, has investigated this phenomenon, notices in his paper one case in which the lower jaw was moved at intervals for two hours (Mr Kestenen) and a second, in which the movements continued in the flexors of the fingers and fore-arm for an hour (Dr

Parkes). We have generally observed these contractions more marked in males than females, owing, in all probability, to the greater muscular development of the former; and certainly the rapidity of the disease to its fatal termination has much to do with them. These post-mortem movements are not peculiar to cholera. Mr Barlow observed them in a case of severe apoplexy fatal after six hours. In all probability, if looked for, they will be found to occur in many cases of sudden or rapid death. "These movements," says Dr Gull, "varied in extent, from flickering and tremulous undulations in a few fibres scarcely to be seen or felt, to contractions sufficiently powerful to move limbs from their position, or even to displace the body itself." "We refer these results," continues the same writer, "to *coincidence* of contraction in different muscles, and not to any co-ordinating influence of the nervous centres." "The rigidity occurred not only at an early period, but lasted from twenty to forty hours" (Gull). As to putrefactive changes, they have generally been regarded as slow. "The bodies examined," says Dr Gairdner, "presented the appearances so well known in cholera, of lividity and collapse, the last being caused by the shrinking of the cellular tissue from the absorption of the watery parts; there was, however, no diminution of fat, which was usually in very considerable quantity in the abdomen and elsewhere. The muscles were of good colour, and usually in strong contraction, from the rigor mortis." The appearance of the bodies of those who have died after reaction has occurred, does not present any peculiar features, and does not, therefore, require any notice here.

Before considering the chief morbid appearances found in the different organs of the body, we shall attend to the accounts which have been published regarding the *state of the blood in cholera*, and the changes that fluid undergoes. This subject has been carefully investigated by several observers, chiefly as regards the chemistry, by Dr Carl Schmidt, Dr Garrod, and Dr William Robertson. As to the physical condition of the blood, its darkness, and little tendency to coagulation are the characters which almost all observers have dwelt upon. "Of the cases fatal in collapse, which are tabulated and affixed to this report," writes Dr Gull, "the state of the blood is noted in 36; in 11 it was dark and fluid; in 5 fluid, with some imperfect fibrinous clots; in 11 it formed soft coagula; in 8 there were dark coagula and fibrinous clots. In nearly half the cases, therefore, the blood was fluid, either entirely so, or with but a few masses of fibrin. In less than one-fourth of the cases did the coagulation approach the normal condition." Dr W. T. Gairdner, as the result of his investigations, has given a somewhat different opinion—" The blood is much less affected in its physical characters than is usually supposed to be the case in cholera; its coagulation within the vessels takes place much as in other diseases. In the majority of instances, firm clots are found within the heart, more or less completely discolorized; and the serum, or non-coagulated portion, contains the greater part of the blood-corpuscles. The colour of the blood presents nothing unusual, the epithets 'dark' and 'venous,' being in no degree more applicable to cholera blood after death, than to that of every ordinary form of fatal disease."[1] The Indian experience of Dr Parkes confirms the facts stated in the table given in Dr Gull's report. " As far as this point (the physical condition of the blood) is concerned," says Dr Garrod, " all recent observations agree with those formerly made, and indicate that, from the commencement of the disease, the fluid becomes more tenacious, of a darker colour, with less disposition to coagulate, and that its specific gravity is very greatly increased." Dr William Robertson ranges, under four heads, thirty-five analyses of the blood—five of which were made by Dr Douglas Maclagan; one by Dr Christison; the remaining twenty-nine by Dr Robertson himself; these heads are as follows:—1. Early stage, *i.e.*, vomiting and purging of characteristic matters, with or without violent cramps. 2. Early collapse, *i.e.*, when to the phenomena of the first stage there succeeds, or is superadded, notable reduction of the temperature of the surface, the pulse, however, being still perceptible at the wrist. 3. Collapse. 4. When patients have rallied from profound collapse, when the pulse has become perceptible, or even strong at the wrist; when the colouring matter of the bile has reappeared in the matters discharged by vomiting or by stool, when the disease is about to take a favourable turn, or after a treacherous amendment to pass into chronic cholera, or into that very fatal stage characterized by continued suppression of urine, by coma, and by visceral congestions.

1*st*, Of blood drawn in the early stage Dr Robertson collected seven analyses. " It was usually obtained with facility, was of the natural colour as it flowed from the vein, and after standing for a few hours, formed a large, easily lacerable coagulum, from which the serum readily separated. In one or two cases there was a thin, buffy coat on the surface of the clot; the serum was generally clear, sometimes milky and turbid. The chief chemical peculiarities were a dilute state of the blood, indicated by a low specific gravity (average of blood 1053·1, of serum 1029·4), by the small amount of total residue left on evaporation, and by the large proportion of water. Upon the whole," says Dr Robertson, "the results of these analyses approximate remarkably to those which stand first in Andral's table illustrative of chlorosis."

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[1] *Monthly Journal*, 1849, p. 903.

2d, Of blood drawn in this stage, six examples were examined. The blood was obtained, in general, without much difficulty; it was of a dark colour as it flowed from the orifice, and coagulated readily on standing; but the separation into clot and serum was slow, and sometimes imperfect. The chemical changes exhibited were, a diminution of water, and increase of solid residuum, by about 25 parts in 1000; the fibrine above the average; the organic solids of the serum increased by about 22 parts in 1000; the globules above the usual standard by about 3 per 1000. "The slight diminution observable in the mineral constituents of the serum" adds Dr Robertson, "is, I think, nearly proportionate to the amount of water supposed to be absorbed from the blood. The specific gravity of the blood averaged 1059·5; of the serum 1033·7."

3d, Blood drawn with difficulty, when fresh drawn always very dark in colour, soon coagulating, often firmly. The coagulum bulky, serum slowly separating, often imperfectly, retaining a slight red tinge throughout, even after being left at rest for eight-and-forty hours. The buffy coat not observed. The chemical changes observed were an average diminution of water, and increase of solid constituents by about 32 parts per 1000, the fibrine was slightly above the ordinary standard, the organic solids of the serum increased by 29 parts in 1000, the salts of the serum slightly diminished. The globules in excess by nearly 3 per 1000. From the milky appearance of the serum in some of these cases, and from a few rough experiments made by agitating the serum with sulphuric ether, I think it probable that fatty or oily matters, possibly some of the bile constituents, were occasionally present in considerable quantity. In one case the formation of oxalates upon addition of nitric acid to some alcoholic extract of blood, led me to suspect the presence of sugar as well as of urea, of which last substance there were unequivocal indications. The mean specific gravity of the serum was 1036·9, that of the blood 1066·3. Several analyses for urea were made by Dr Maclagan of the blood taken from the dead body. In one case only did he fail to obtain the characteristic crystallization upon treating the evaporated alcoholic extract with nitric acid.

4th, Blood drawn in this stage flowed readily, was of the natural colour, coagulated firmly, in some cases became covered with a buffy coat, and soon separated into clot and serum. The proportion of water and solid blood here vary very slightly from Lecanu's standard of health; the fibrine is increased 7 per 1000; the organic serous solid, and the globules are in almost normal proportion; the salts of the serum are present in smaller quantity than in any of the earlier stages of the disease. The specific gravity of the serum is 1030·8, that of the blood 1055·8. Urea was found in the blood of every case fatal in the reaction stage, to which the tests were applied. Upon a reconsideration of the results of his valuable and most accurate experiments, Dr Robertson thinks the following conclusions warranted:—"1st, That anæmic subjects are more prone than others to attacks of cholera. 2d, That it is shown by the first three tables, that the changes which so rapidly affect the blood during cholera, are concentration of the serum and globules, from the abstraction of water, together with a proportionate loss of salts. 3d, The changes subsequently effected during the stage of reaction are far more complex. One of these, and the chief one, is that water seems to pass rapidly into the circulation, diluting the serum and causing a notable diminution in the proportion of its salts." The following are Dr Garrod's conclusions, after his numerous experiments. "In comparing the recent analyses with those made during the last epidemic, it will be seen, that as far as concerns the physical properties of the blood, the diminished amount of water, and the consequent increase of the solid portion, also the high specific gravity of the serum, and its tendency to become less alkaline, our own conclusions perfectly agree with those previously made; and therefore the conclusions 1, 2, and 3 are confirmed. These are—1. That in cholera, the physical characters of the blood are altered, and that its tendency is to become thicker, tar-like, and less coagulable. 2. That the proportion of water is much diminished. 3. That the specific gravity of the serum is very high, (1039 and 1041, [Garrod]. 1036·9 [Robertson] as compared with 1028, healthy serum[1]), which is due to the increase of the solid portion of the serum, and especially of the albumen; and that this fluid also tends to become less alkaline in its reaction. Regarding the salts and urea, Dr Garrod adds the two following conclusions:—4. That, in cholera, the saline constituents of the blood are not only not decreased in amount, but sometimes exist even in increased proportion, and that the diminution of its alkaline reaction is not due to the loss of salts, but to the impeded excretions of organic acids, which are constantly being formed in the system. 5. That urea usually exists in increased quantity in cholera blood, but that the amount differs considerably in different stages of the disease; being but small in quantity in the intense stage of collapse, increasing during reaction, and in excess when consecutive febrile symptoms occur.

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[1] Sp. gr. of the blood in males 1076 and 1081; and in females 1068, 1074, and 1076; in children 1076, (Garrod), 1066·3 (Robertson). The maximum sp. gr., according to Bequerel and Rodier, being 1062 (male), 1060 (female).

Condition of the Alimentary Canal and its Secretions.—No uniform or even frequent morbid appearances have been met with in the œsophagus, pharynx, or stomach, though in certain individual cases lesions sufficiently remarkable have been described, of which injection, congestion, and destruction of the epithelial membrane in the two former are most noticed; and, as regards the stomach, distension, softening of the mucous membrane, venous injection, ecchymosis, and enlargement of the solitary glands. As a general rule, these organs appear to have been found healthy; at all events, their examination has not thrown much light on the morbid anatomy of cholera. It is otherwise with the intestines; for, while it is quite true, as Dr Gairdner has remarked, "that it was by no means uncommon, especially in early fatal cases, to find the intestines throughout natural in colour and appearance, or even paler than natural, and in many of these cases no prominence of the solitary glands," the existence of these conditions, more especially the latter, along with the ecchymotic patches, are now justly considered as nearly distinguishing morbid appearances in cholera. "The most frequent of all the abnormal conditions of the mucous membrane," writes Dr Gairdner, "was the prominence of the intestinal glands, both aggregated and solitary, but especially the latter. This condition, the '*Psorenterie*' of some French writers, was found in about two-thirds of the cases." "The glandular structures," says Dr Gull, "were the seat of changes similar to those observed in the mucous membrane itself. The solitary glands were almost universally distinct, especially in the lower part of the ileum." "The changes in the patches of Peyer," according to the same author, "were similar to those occurring in the villous surface of the mucous membrane generally, and in the solitary glands. The appearances presented by these structures are thus described by Reinhardt and Leubuschern. The patches of Peyer were almost constantly altered, and most remarkably so in the lower part of the ileum, immediately above the ileo-cæcal valve; sometimes they were the seat of intense capillary hyperæmia, which either included the whole group or only a part. More commonly such hyperæmia did not exist, and the patches were remarkable for their whiteness. In many cases we observed a more or less prominent swelling of the separate follicles, without thickening of the intervening mucous membrane. The patches had then a granular appearance, and the separate follicles could be distinguished. These had undergone the same changes as the solitary glands. More commonly there was found a moderate infiltration of the two structures of the patches (glands and mucous membrane) with a white solid exudation, which, as Böhm has particularly described, extended to different depths into the submucous cellular tissue. The patches then formed more or less prominent areas of a round, elongated, or irregular form. The surface was in general smooth, but sometimes the mucous membrane was raised into folds and rounded masses. The exudation in all these cases was solid, and appeared under the microscope as an amorphous finely-granular mass, becoming transparent under the action of acetic acid and alkalies." Regarding the commonly-noticed ecchymosed patches in the course of the intestines, Dr Gairdner has observed—" They were distinctly circumscribed in tint, varying from claret colour to the deepest purple, approaching black, but in parts not unfrequently greenish or ash-coloured. Their surface presented a similar appearance, in all but colour, to the rest of the mucous membrane, and was not perceptibly elevated. In these instances, however, a different appearance was observed in the colour, the mucous membrane presenting a few flattened elevations, each over the extent of about a sixpenny piece, of a grayish or leaden colour. These elevated patches were evidently due to a submucous exudation (probably identical with the diphtheritic exudation described as occurring, though not always equally frequent or extensive, in cholera by Virchow[1]), and yielded on section a creamy fluid.

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[1] The changes in the mucous membrane, according to Virchow, are similar to those produced by the different gradations of catarrhal and diphtheritic inflammation. The latter begins with intense hyperæmia, which is quickly followed by extravasation into the tissue of the gut and into its cavity (hence the bloody stools); subsequently there follows an exudation into the superficial layers of the mucous membrane, at first small in quantity, and of a grayish-white colour, but becoming more opaque as it augments, and acquiring a yellowish tint by the imbibition of bile. Examined microscopically, it appears

In this fluid microscopic examination showed no well-marked pus corpuscles, or complete cells, but a number of nuclei, on which acetic acid produced no effect. In one protracted case, in which the great intestine was much ecchymosed, there were traces of dysenteric lymph on several parts of the mucous membrane. In two other instances, there was distinctly exudation of yellowish lymph, with heightened vascularity on the external serous surface of several folds of small intestine." The exudation of dysenteric lymph in the large intestine we have recently observed in two cases—in both of which the stools had been bloody. Neither case, however, could be properly called protracted. The matters found in a greater or less quantity in the intestinal canal bear of course a very close resemblance to the peculiar cholera evacuations;—their chemical constitution and microscopical character we shall immediately allude to. The peculiar condition of the epithelial covering of the intestines, and the existence of a large quantity of epithelial cells in the fluids found in the intestines, led Böhm in 1832-33 to attach great importance to the appearances presented by the denuded villi of the intestines, and to regard the desquamation of the epithelium as a really important pathological condition. Dr Parkes and Dr Gairdner have taken a very different view of this matter, regarding "the presence of epithelium in such large quantities in the fluids found in the intestines after death, as the result of purely mechanical macerations upon a mucous membrane to which, during life, the epithelium remained attached." The reasons Dr Gairdner assigns for this view appear quite conclusive. 1*st,* That artificial maceration produces a similar result on a healthy mucous membrane. 2*d,* That the epithelium found in the fluids of each division of the intestines is always that of the particular part where it is found. 3*d,* That the true cholera stools, *passed during life,* contained so little perfect epithelium, that it cannot be considered as anything more than an accidental ingredient."

The conclusion of Dr Lauder Lindsay upon this subject is to the same effect,—"That the phenomenon in question has no claim to be considered as *vital,* or as standing in any essential relation to the disease, but is probably purely *mechanical,* viz., the result of *post mortem* maceration of the mucous membrane."[1] Mr Mahony,[2] adopting the views of M. Mialhe, describes, "a viscid pulp sometimes observed

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as a granular amorphous deposit in the cellular tissue. The parts thus affected soon begin to undergo disorganisation, leaving an eroded or superficially ulcerated surface.—*Dr Gull's Report,* page 21.

[1] Association Medical Journal, p. 527.

[2] On Cholera, p. 74.

in patches, and frequently so adherent to the inner surface of the stomach or intestine, that it would appear to be almost a partial solution of the villous coat. But this we conceive to be the albuminous matters arrested in their passage into the digestions—the result of muscular relaxation and contraction upon the defective assimilation under the choleraic influence." We are not quite sure that we understand Mr Mahony's theory any better that he appears to understand M. Mialhe's (to which we shall presently have occasion to allude); but at all events, we do not require to insist further on the very simply demonstrable and now very evident facts which Dr Gairdner has adduced. The peculiar appearance of cholera discharges is well known; upon standing a separation takes place into a clear fluid, and a flocculent sediment. The specific gravity of the former was from 1006 to 1010, rarely higher (Gull), 1005 to 1010 (Gairdner). Boiling produced only a slight opalescence. Dr Parkes gives the following details as the mean of his observations:—Water 987·95, solids 12·05, in 1000 parts. The solids therefore, which consist according to Dr Parkes of soluble salts (chlorides, phosphates, and sulphates of soda and potash) 8·1, and organic matter and insoluble salts (earthy phosphates) 3·9, form a very small ingredient of this fluid. The reaction of the evacuations was always alkaline or neutral. The presence of albumen in the fluid portion as tested by heat and nitric acid, is far from invariable; indeed, Dr Gairdner usually found the presence of albumen to coincide with that of a small quantity of blood. "The alkalinity of the fluid, and probably also the physical condition of the albumen allied more to albuminose, prevented its ready recognition by heat" (Gull). Nevertheless, albumen, in all probability, is a constituent of the evacuations. Dr R. D. Thomson states "that on evaporation *in vacuo* a yellow residue was obtained, which, on being heated with water, yielded a solution coagulating on boiling, and by the addition of acids." The same observer states that, "usually the fluids as evacuated, when allowed to settle, yielded by boiling, or on the addition of an acid, distinct evidence of the presence of albumen." The experiment of the evaporation *in vacuo* speaks for itself; but the latter statement certainly shows a very different experience from our own.

M. Mialhe calls this substance *albuminose*, and regards it as "the ultimate product of the digestion of albuminous substances." Its presence in the blood, as yet unestablished by experiment, lies at the base of his theory of cholera. Dr Gairdner, holding in remembrance the close chemical relation-ship subsisting between mucus and the proteine compounds, truthfully as scientifically, calls it mucus. Dr Parkes has directed attention to the important point of the very limited presence of "extractives or incoagulable organic substances," met with in the evacuations, their absence seeming "to indicate a suspension of the proper excreting function of the mucous membrane during the algide stage" (Gull). On the addition of a little nitric acid a pink or rose colour tint was communicated. This Dr Parkes at one time did not allow to be owing to the presence of bile.[1] Now, however, he and most other observers regard the existence of "modified bile" as its occasion. The presence of urea in the cholera evacuations has been frequently demonstrated. Garrod, on one occasion, detected very faint traces of uric acid. When a portion of the denser part of the cholera evacuations is examined under the microscope, the following appearances are noted,—molecules and granules, nuclei and cells, a few blood corpuscles (not common). The nuclei are from 1·250th to 1·180th of an inch, and are not affected by acetic acid. The cells are those of columnar and scaly epithelium; in the evacuations these are few in number, affording a striking contrast to the large amount in which they are detected in the fluids found in the intestines after death. On evaporation by exposure to the air, a large number of crystals of chloride of sodium, in tubes and octahedra, and occasionally some crystals of urate of ammonia and urate of soda; and when decomposition had commenced, prisms of triple phosphate were seen (Gull.) In regard to the other abdominal organs:—1. The spleen. No very constant pathological appearances have been noticed; but ecchymosed spots on its surface, and limited congestions in its substance, are of these the most common. 2. The liver and gall bladder. The existence of old lesions in the liver of those who have died of cholera has not been uncommon; but from such as might have been established during the progress of the disease, this organ, like the spleen, has been singularly exempt. The gall bladder has, for the most part, been found distended with dark bile, generally tenacious. The mucous membrane of the gall bladder and ducts has frequently been the seat of ecchymosed spots, but otherwise healthy. "In the late epidemic," says Dr Gairdner, "in every instance except two, the gall bladder contained more or less bile, which was mostly of good colour and consistence, and, in the greater number of instances, in quantity sufficient to produce considerable distension. One of the exceptional cases was the one formerly referred to, of diseased liver, in which the gall bladder contained only a thin coloured fluid; in the other, abundance of bile was present in the duodenum and stomach. The gall ducts were most frequently empty, but occasionally contained bile. A probe passed in all cases easily from the duodenum to the gall bladder, being usually somewhat tightly grasped at the neck of the latter, as is invariably the case, whatever the cause of death may be." 3. The kidneys. "According to Reinhardt and Leubuscher, quoted by Dr Gull, "the kidneys had frequently, even in the first stage of the disease, undergone a change. The vessels, especially the veins, were slightly hyperæmic, but there was never the same extent of capillary engorgement as in the latter stages. Even in those who died in the algide stage, we not rarely found portions of the secreting structure whitish or yellowish-white, from an exudation into the tissue." This change, as Virchow has proved, began at the apices of the pyramids, and thence extended towards their bases. The papillæ, and a greater or smaller section of the pyramids, were compact, and of a whitish colour. Before the infiltration had become extensive, similar changes usually began in the cortical portion, sometimes near the surface, but more commonly at the base of the pyramids, thus surrounding them with a layer of a gray or whitish-yellow colour. As the disease advanced, the infiltration of the renal structures became more extensive, and affected the cortical substance, which was increased in thickness, of a light gray colour, and easily lacerable. On stripping off the capsule, the surface was left uneven and granular. The enlargement of the kidneys accompanying these changes was invariable, and occasionally remarkable.

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[1] See an interesting paper in Monthly Journal, 1849.

The epithelial cells of the uriniferous tubes in the discoloured parts adhered more firmly together than in the normal state. In certain cases, solid exudation into the tubes was found. The researches of Drs Gairdner and Borthwick on this subject appear to have established this point, that an excessive quantity of oleo-albuminous exudation has taken place into the kidneys. "The cortical substance was pale and turgid, and the tubuli uriniferi gorged with imperfectly developed epithelium, which was mostly loaded to an unusual extent with oleo-albuminous granules" (Gairdner). 4. Urinary bladder. No more marked feature is found on the dissection of cholera bodies than the contracted state of this organ. At the same time, we have ourselves seen this condition nearly reversed; the bladder, if not distended, at least containing some ounces of urine, a circumstance which, during life, had never been suspected.[1]

5. Female sexual organs. The uterus, according to Reinhardt and Leubuscher (Gull's *Report*, page 36) in the cold stage, exhibited the following appearances. Its contents were mostly sanguineous, sometimes mucus tinged with blood, and sometimes blood only, either fluid or coagulated. (These authors speak of sanguineous discharges from the uterus during cholera). Hyperæmia sometimes universal, but more frequently, when intense, limited to certain parts, namely, the fundus, the upper part of the body, and the neck from the os externum to the middle. In those who died in the typhoid stage, the mucous membrane was most commonly intensely hyperæmic, with extravasation of blood of variable extent, or, still more frequently, the whole organ was uniformly infiltrated with blood." The vagina and ovaries are also described as partaking in the same appearances. So far as the condition of the uterus just described is concerned, we are disposed to think that the observations in this country will scarcely admit the existence of so excessive a hyperæmia; no doubt, the condition of the uterus itself at the time of the patient's seizure has much to do with these changes. *Lastly*, the peritoneum. Injection of the small capillary veins, more or less intense, and distinct ecchymotic spots have been described; but neither of these conditions have been so frequent as the general dryness of this and the other serous membranes, which has attracted universal attention.

The condition of the lungs in cholera is not remarkable. Sometimes engorgement, with dark blood, of the dependent portions existed. The bronchial mucous membrane and the pleura were not unfrequently the seat of ecchymotic spots. The pericardium, like the peritoneum, was generally dry; and though occasionally it contained a small quantity of serum, as a general rule it was empty;—this fluid, generally straw-coloured, was in some instances found to be sanguinolent (Gull). Ecchymosis on the pericardium and over the base of the heart was very common. The condition of the brain and membranes:—These, with the exception of a loaded state of the veins and sinuses, presented no marked morbid appearances. As to the nervous system generally, attention has often been called to some supposed important lesion of the sympathetic ganglia and the pneumogastric

[1] When death has occurred after reaction has taken place, urine is very frequently found in the bladder.

nerves; but though both were frequently examined (Gairdner), nothing unusual, except in a few cases slight ecchymosis, was discovered.

When death has occurred after *reaction* has taken place, a certain difference, in some at least, of the morbid appearances described, has been found. Attention has already been directed to such of these as appear of real importance in relation to the disease. But, as a general rule, the differences we allude to chiefly concern the lungs and brain, and are rather illustrative of the particular manner in which death occurred, than of importance in connection with cholera. We need not, therefore, devote time to their discussion.

There remains for our consideration the pathology and treatment of cholera, to which we propose to devote attention in a separate paper, with more particular reference to our own observation of the disease in the Cholera Hospital of Edinburgh during the late epidemic.

PART II.

CRITICAL ANALYSIS.

ART. I.—*Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D. Third Edition. London: 1854.

The profession does not need a reviewer to announce that a scientific work on the diseases of children, which has gone rapidly on to its third edition, is a valuable one. Nor does the reviewer, in the present instance, attempt to demonstrate it. The book is full, elaborate, copiously illustrated with clinical observations, and altogether worthy of the high place it has already attained. Moreover, it is a growing and improving book, because its author is a laborious and improving man.

Nothing is more foolish than to give unqualified praise. Although we have nothing to say in dispraise, we shall briefly indicate some points where we would comment, if we intended to review. Our object is merely to name these, and then to give a brief sketch of what appears to us to be the most important recent advance in this department of medicine.

The book should be weeded of two or three reference (which are not references), to plates seen (which are not seen), such as occur in pp. 38, 39. It is, at least, funny to print these words of reference to plates exhibited "on the board." Dr West surely has not these words in his MS. lectures even,—why then have them in his book? If he wishes the public to have a fac-simile of his lectures, he should have begun them all with "Gentlemen: At the close of last lecture," &c., and ended in like manner.

After these petty remarks, we have two observations of a graver nature to make. First, Dr West in his book (just as happens in all hospital practice), sees and describes few or none but severe cases. He in general gives far too high an impression of the gravity of the run of cases of the malady he describes, and thus, to a great extent, ignores nature and truth, in which we find a great mass of slight cases of almost all affections. Second, and as a necessary result of his first error, Dr West recommends very energetic treatment. Bleeding, purging, calomel and antimony are profusely given. The ordinary reader is made too anxious about his case, and is recommended to treat too vigorously. If Dr West announced that he only described the severest forms of disease, then all his advice would be safe. But it is not so. In evidence of our statement we would refer to the chapter on the palsy of the limbs of infants, where the prognosis given ignores a great class of slight cases in which the practitioner is consulted, for the parents are very anxious;—the treatment recommended too is such as is, to say the least, quite inapplicable. Indeed, the treatment of Dr West is altogether pitched on a higher and more heroic key than we see resorted to here in any class of cases.

In reading through the work, we meet with much theorizing, which strikes us at once as fanciful, and not adapted to paint a subject or point a practical rule,—the chief uses of such reasonings in a book like that before us.

One of the most decided steps in advance which has recently been made in any department of scientific or practical medicine had its first beginnings in the researches of continental pathologists on the pulmonary affections of newly-born children. The labours of Jörg demonstrated the non-inflammatory nature of that state of the lung which he described, under the name of Atelectasis. He has shewn that this affection is in no sense a form of pneumonia or bronchitis; but, on the contrary, is a result of weakness and deficient inspiratory efforts in the newly-born child, part of the lung remaining in its fœtal state, undeveloped and uninflated from this cause. In such cases it may happen, that the imperfect condition of the respiration may result in death. On examination after death, the solid parts of such lungs sink in water, but they are not friable, and contain no inflammatory exudations. They are distinguishable from hepatized lungs by the experiment of artificial inflation, which distends the air-cells by a power greater than the weakly child could exert, and renders them buoyant in water.

> "If you examine the body of a new-born infant, or of one that has survived its birth but a few days, you will sometimes find patches of the lung of a dark red colour, and depressed below the surrounding tissue, thus giving to the surface of the organ an uneven appearance. These darker portions, which exactly resemble fœtal lung, are solid to

the touch, do not crepitate at all under the finger, and sink immediately if thrown into water, while no minute air-bubbles are intermingled with the small quantity of reddish serum which pressure causes to exude from their divided substance. They are not friable nor easily torn, their cut surface is perfectly smooth, closely resembling a piece of muscle, and, if examined under a lens, the pale collapsed air-tubes are seen intersecting their substance, and scarcely distinguishable from the small vessels, which are almost devoid of blood.

If air be blown into a lung some lobules of which have this appearance, it will permeate the collapsed air-tubes; the pulmonary vesicles will by degrees become distended, and the solid lobules will rise to a level with the rest of the lung, will acquire the same colour and consistence, and like other parts of the organ, will float in water."

In connection with this condition of atelectasis it is common, as Dr West admits, to find that the new circulation after birth has not been completely established. The pulmonary arteries are found to contain less blood than usual, and the foramen ovale and ductus arteriosus retain greater patency than ordinary. These circumstances appear to us to justify, in a great degree, the opinion of Jörg, that precipitate labour is a cause of the condition, and also his theory advanced in explanation, a theory not so fanciful by half as many which are adduced and blamelessly so, in this very work; a theory too which we believe the study of rapid labour will amply justify. Before we were acquainted with Jörg's remarks on this subject, we had more than once stated in lecturing on too hasty labour, the disadvantage to the fœtus, from *unpreparedness* for entering life in the same terms as Jörg ably uses in support of his views.

"Dr Jörg has (says Dr West) attributed great importance to precipitate labour as a frequent cause of its occurrence, and has suggested a somewhat fanciful theory to explain its mode of production. He conceives that one grand use of the uterine contractions is gradually to enfeeble the circulation through the placenta, and thus to induce in the fœtus that *besoin de respirer* which shall excite the complete establishment of respiration immediately on its birth. If, however, by the very rapid course of labour, the child should be born while the fœtal circulation is still going on with unimpaired vigour, the want of air will not be experienced by the child, and its attempts to breathe will be feeble and imperfect."

It was not long before these discoveries were used, to throw light on the pathology of children and adults. For this advance, science was in a great degree prepared. "In 1829" (says Dr Gairdner, *Pathology of Bronchitis*, p. 16), "M. Louis described the condition of the lung in a variety of cases of typhoid fever. These descriptions were repeated in his work on fever in 1841, in which the state of the lung in fevers was compared with that found in other diseases. The result of these inquiries was a most accurate description of the collapse of the lung as a state altogether different from pneumonia, and which was found not only in typhoid fever, but in a variety of other diseases; chiefly at the posterior part of the lung, but sometimes *disseminated* (lobular). M. Louis offers no speculation or remark as to the origin of this affection.[1]

"In 1830, Dr Alderson, in a paper on the 'Pathology of Hooping-Cough,'[2] pointed out most clearly the distinction between the lobular condensation observed in that disease, and true hepatization of the lung. 'In hooping-cough, the lung is always dense and contracted, as if the air had been expelled, and from the throwing out of adhesive matter, the sides of the air-cells had been agglutinated together, while in hepatization the lung is less dense than in hooping-cough, and is rendered more voluminous than in its natural state.'[3] The inflation of the lungs, would probably, in some cases at least, have clearly disproved this supposed agglutination of the air-cells."

These two distinguished pathologists failed to recognize the true pathology of the morbid appearances they carefully described, the conditions of atelectasis being almost unknown at the time they wrote. So late as 1838, MM. Rilliet and Barthez, in their great work on children's diseases, continued to describe the secondary atelectasis or collapse of the lung under the old and misleading name of lobular pneumonia. And it was not till about ten years ago that Bailly and Legendre demonstrated the identity of the pathological lesion in the cases misnamed lobular pneumonia of children, with those of the atelectasis of Jörg. In the former set of cases, the lung is said to be collapsed or to have returned to its fœtal condition; in the latter cases it has never been developed. In this lies the distinction between collapse and atelectasis. In both cases the lung is devoid of air in its cells, and in both air can be artificially injected so as to make them float in water. This collapse of previously inflated lung may, in children, be the result of debility, and specially of diminished force in inspiratory efforts, or it may result secondarily from other affections of the lung, especially bronchitis, in which disease the morbid secretions may, as

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[1] Recherches sur la Gastro-enterite, i. 361; and Recherches, &c., sur la Fievre Typhoide, i. pp. 328–334. It is worthy of remark that, from the observations in the preceding pages on the Edinburgh epidemics of 1847, as well as from Dr Jenner's recent careful descriptions of cases observed in London (Monthly Journal, Feb. 1850, p. 115, et seq.), this form of pulmonary lesion does not appear to be peculiar to any type or form of a continued fever, at least in this country.

[2] Medico-Chirurgical Transactions, vol. xvi., p. 78.

[3] *Loc. cit.*, p. 91.

Dr Gairdner has shewn, come to act mechanically in the air-tubes by preventing ingress of air to a portion of lung, which will then collapse.

"Appearances," says Dr West, "supposed to be the result of pneumonia, had long attracted the notice of writers on diseases of children, by the wide differences which they presented from those which inflammation of the lungs gives rise to in the adult.[1] It had been observed that infants and children under five years of age often died after presenting some of the symptoms of inflammation of the lungs, such as cough and difficult breathing, together with more or less extensive dulness of the chest on percussion, and some or other of the auscultatory signs of solidification of the lung. In such cases these peculiar morbid appearances were especially well marked. But while they seemed to prove that these changes in the lung were the consequences of pneumonia, it happened not infrequently that the fever and the pneumonic symptoms underwent a great abatement before any sign of approaching death appeared, or that children who had seemed to die worn out from various causes, and during whose lifetime no indication of inflammation of the lungs had existed, presented the supposed anatomical evidences of pneumonia in a most remarkable degree. The frequency of occurrences of this kind led to the assumption that pneumonia was an extremely frequent concomitant of almost all the diseases of infancy and early childhood; that this pneumonia was very often latent (that is to say, that it did not manifest its existence by those symptoms which usually attend it); and lastly, that, owing to causes which were differently stated by different observers, it gave rise to alterations in the lung very dissimilar from those which it occasioned in the adult."

It was reserved for Dr W. T. Gairdner to apply these new and rich discoveries to the pathology of the adult. In his papers, originally read to the Medico-Chirurgical Society of Edinburgh, he has not only effected this object, but has added numerous original researches on bronchial abscess, on the cause of emphysema and of cardiac dilatation. These researches are not barely scientific in their ends, but already afford us the most perfect solution of old practical difficulties, and consequently valuable therapeutic indications when such difficulties recur. On this subject, we have the following passage from Dr West, which is true as well of the adult as of the child.

"The course of the disease (collapse or carnification of the lung), in many of these cases during the lifetime of the patient, and the results of medical treatment, tended to enhance the difficulties which the above-described anatomical peculiarities placed in the way of referring lobular pneumonia to the same category of affections with the pneumonia of the adult. Venesection, leeches, and mercurials, the ordinary antiphlogistic apparatus in the pneumonia of the adult, often appeared to hasten the child's death; blisters rarely effected any good, and the blistered surface often showed a remarkable indisposition to heal. On the other hand, emetics and rubefacients were frequently of service; a stimulant plan of treatment was almost always necessary at an early period, and sometimes seemed to be required nearly from the outset of the affection. The rapidity of the changes that took place in the physical condition of the lung was another peculiarity which rendered the nature of the affection still more obscure; for where air was heard entering freely on one day, none would be perceptible on the morrow, but percussion of that part of the chest would yield a sound of complete dulness. On the other hand, it happened sometimes, though much less often, that dulness was succeeded just as quickly by resonance on percussion, and that breathing became distinctly audible where on the previous day no sound of air was to be heard.

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[1] In the British and Foreign Medico-Chirurgical Review for October 1853 is a very clear and interesting sketch, by Dr Willshire, of the progress of knowledge with reference to pneumonia, and conditions of the lung resembling it in early life.

" Nothing can show more forcibly the influence of a name, than the fact that this condition of the lungs should have been described by all writers as lobular pneumonia, and that its symptoms should have been attributed to inflammation, while yet it was evident from the concurrent testimony of every one that neither in its progress nor in its results was it similar to inflammation of the lungs in the adult, much less identical with it. Having, however, once been called pneumonia, every person continued to call it so, though often with a full recognition of its peculiarities."

In regard to the subject of emphysema, which Dr West does not treat of separately as a distinct affection, we do not find the views casually expressed to tally with our own notions on this subject. The theory of emphysema is now, it appears to us, from the investigations chiefly of Dr W. T. Gairdner, indissolubly bound up with the occurrence of collapse or other atrophic disease of the lung. At page 246 we find Dr West connecting, by "a marked relation," the extent of pneumonia with the extent of emphysema in the same lungs. To be brief, we shall here merely say that the researches of the Edinburgh pathologist demonstrate that, at least in a large class of cases, this statement is inconsistent with fact, as it is undoubtedly inconsistent with his new and unassailed theory of the production of emphysema. Perhaps there is here, in Dr West's mind, a confusion of observations made in children at a time when he and others confounded real pneumonia with pulmonary collapse.

" Emphysema is," according to Dr G.'s theory, " *an increase in volume* of those portions of the lung to which the air has access, to supply the place of *diminished volume* in those parts from which it is excluded. It is produced by the expansion of the chest in inspiration, and is dependent simply upon the normal expansion-force being exercised under the abnormal conditions to which we have alluded. It cannot be produced, in health, by *any* amount of inspiratory violence, because the lung admits of being readily and easily expanded, without straining any of its air-cells, to the full volume permitted by the expansion of the thoracic walls. It cannot even be produced in disease except when the volume of the lung is directly diminished in relation to the space which it has to fill in inspiration. Emphysema, therefore, is never found in connection merely with pleuritic effusion, or with hepatization, or with tubercle. It cannot be produced where large cavities exist in the lung, even in connection with atrophy, if they have very flaccid walls, and be distributed through all its lobes; for under such circumstances the cavities are expanded by the inspiration-force more readily than the air-vesicles can be forced beyond their normal maximum. Hence the comparative rarity of emphysema in connection with rapidly advancing tubercle, while it is generally observed to be the accompaniment of the retrograde or contracting stages of that affection. Emphysema is, on the other hand, certain to be produced wherever considerable atrophic lesions exist (especially if they are scattered and irregular in their distribution), provided the inspiration-force be exercised to the full extent; that is, provided the strength of the patient, and the physiological need of respiration, be not seriously impaired, as it often is in debilitating diseases; provided also the volume of the chest be not materially encroached upon by causes acting from without, as in the case of ascites or pleuritic effusion. All of these are circumstances which restrain or avert the development of emphysema of the lung, even under the conditions that would otherwise produce it."

In conclusion, we would point out that, on finding emphysema present in autopsies, we must not expect, as Dr West seems to do (Note, p. 339), always to discover some palpable atrophic condition of the lung present. That may have disappeared from the progress of curative changes in the lung, or its last traces may have been removed by advancing atrophy of the morbid part. Indeed, as Dr Gairdner has remarked (see Pathology of Bronchitis, p. 68), " simple atrophy of the lung, in its most complete form, is a lesion only to be distinguished by negative characters. The proper and special elements of the pulmonary tissue have disappeared, but they are not replaced (as in atrophy from other causes) by any adventitious structure, or even by the thickening or induration of the fibrous basis. For this reason, simple atrophy is sure to be overlooked, unless its traces be sought for in the manner I have described. In the centre of a lung very many lobules may be entirely atrophied, and leave no visible or tangible evidence of their previous existence." In such cases we may find emphysema as the sole lesion in the lungs, or find it accompanied with intercurrent non-atrophic affections.

In closing our notice, we must again recommend Dr West's book as one of very high merit, and the best on the subject in the English language.

ART. II.—*Psychological Inquiries, in a series of Essays intended to illustrate the mutual relation of the Physical Organization and the Mental Faculties.* 12mo. pp. 264, LONGMAN: London, 1854.

It is no secret that the little work before us is from the pen of Sir Benjamin Brodie. It rejoices us to see a veteran of his high standing bestow the well-earned leisure of his later days on a subject so full of important consequences to the future welfare of society as psychological inquiry. We should prize the example set by such a man as Sir Benjamin, were the contribution of less value than it actually is. For we boast to be of the number of those who think that psychological inquiry has much good in store for after generations. And we feel satisfied that it is by the thinking men of our own profession that no small share of that good must be wrought out.

The departments of knowledge which have been hitherto carried to the greatest height of advancement are those which either admit of being subjected in their conclusions to the tests of number and quantity, or accommodate themselves to a nomenclature, representing objects of sense, so that as often as a misapprehension anywhere occurs, it is susceptible of a ready correction. All mathematical research, however profound, is but an immensely enlarged development of the primitive reckoning by measures and counters. Nearly all physical phenomena are reducible to sensible experiment or to calculation. The several departments of natural history, for the most part, rest on the exact observation, by the senses, of appearances and of qualities with or without the aid of instruments. But there are numerous departments of human knowledge, the language of which, whether as respects the particulars on which their essential principles are founded, or the inferences deduced from such principles, is descriptive solely of ideas of the understanding, which refuse to conform to any standard of sense. Such departments may be described, in a large way, as resting on the metaphysical. All these branches of knowledge are very backward in their progress at present, as compared with physical science and natural history, and among them unfortunately stands a great part of medicine. If the cultivation then of psychology can finally habituate men to the exact use of language which does not refer to sensible things, how many doubts, difficulties, misapprehensions, discussions, and controversies would be saved! The mere study of psychology or metaphysics does not necessarily convert a man who is naturally a bad reasoner into a clear-headed person. But few men reason badly when they understand all the import of the terms they use. It is not understanding, so much as exact knowledge, when sensible images fail, that is wanted. In his first Essay or Dialogue, Sir Benjamin makes the speaker who represents himself remark that "the faculty of reasoning correctly (or what is commonly called a clear head) is for the most part a natural gift, and that it admits of being artificially improved only in a limited degree." We would suggest to Sir Benjamin that the cause why so few, who have not the natural gift of reasoning, can be taught, is that no attainments in the rules of reasoning can compensate for a defect in the appreciation of the nicer shades of language on the subjects under discussion. We cannot but regard the cultivation of metaphysics as the best introduction to the use of language as an instrument of thought and reasoning on subjects not referrible to sense.

Metaphysics, it is true, have never yet been cultivated to any great general good among men—the effect of their general cultivation in former ages was to carry wrangling upon absurd topics to an extravagant extent. But that was at a time when the necessity of accumulating useful facts was little understood. The artist displayed his skill upon frivolous topics, because the materials to work up into productions of value were not supplied to him.

If, as is not unlikely, the physical sciences shall begin ere long to afford less scope for the exercise of human genius and industry, it may be confidently anticipated that man's mental energies will become more generally turned to those departments of knowledge which possess a psychical character, and that the world will then learn to appreciate the real value of metaphysical pursuits. Of the wide-spread capability among men for the profoundest metaphysical researches no doubt need be entertained; let the inducements only arise for its prosecution. Of this, no one at least can doubt who is a convert to the belief, that the most signal development of the metaphysical capabilities of the human mind which the world has as yet seen, took place under the most disadvantageous circumstances as to study, namely, in those times which owing to the clouds that intercepted the diffusion of knowledge among the people, are truly termed the dark ages. That it was fruitful in words, but barren of works, Sir William Hamilton says, "is the vulgar opinion in regard to the scholastic philosophy. The few are, however, now aware that the human mind, though partially, was never more powerfully developed than during the middle ages."[1]

Sir Benjamin describes his inquiries "as intended to illustrate the mutual relations of the physical organization and the mental faculties." A work confined to this end is not strictly a metaphysical work, but a contribution from physiology to metaphysics, and this description, better than any other, truly denotes the character of Sir Benjamin's book. It is obviously in this line that the medical profession can most profitably aid in the advancement of metaphysics. Dr Reid, as Sir Benjamin remarks, by his work on the senses, was one of the first among metaphysicians to set the example of combining the inquiries proper to the metaphysician with those which belong to the physiologist.

It is the subject of sensation which is common ground to the metaphysician and the physiologist. And here unquestionably the conclusions of the physiologist are capable of throwing much light on the speculations of the metaphysician. The metaphysician recognises sensation as one of the most prominent phenomena of consciousness, but all that he knows thereupon beyond what he is taught by reflection on the subjects of consciousness, he learns from the physiologist. He is not entitled to claim for his own science even the belief that the brain and nerves are concerned in sensation—that he knows only through physiology. Physiology, however, can teach much more than this regarding sensation—and it is high time that a formal inquiry were instituted to ascertain how far the established conclusions of physiology on the subject of sensation are applicable, if not to the solution, at least to the more clear statement and apprehension, of the great problems which lie still unsolved at the very threshold of metaphysics. Of this character are the questions regarding the origin of our notion of space, of motion, of the external, of substance, &c.

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[1] Reid's Writings, with Hamilton's "Notes," p. 268.

The physiological facts or conclusions applicable to such questions may be stated in the following terms: that every sentient nervous filament is an independent instrument of sensation, and that every sensation involves an element of space, namely the point of space where the organic change produced by the impression originates, that is, at the peripheral extremity of the nervous filament concerned.[1] Sir William Hamilton—one of the metaphysicians who in our time has most clearly perceived the advantages of conjoining physiological with metaphysical investigation—refers to this law of sensation in the following terms: "The physiological law is, that a nervous point yields a sensation, felt as locally distinct, in proportion as it is isolated in its action from every other."[2]

The question, then, arises—Does it not follow from this law, even as stated by this distinguished metaphysician, that the infant in no long time after birth must come to recognise the relative position of all the sentient points in the space occupied by the skin and the muscular system; and therefore that it must then become cognisant of the space as a whole in which these two systems exist—that is to say, must become cognisant of the chief part of the form of its body. By the affirmation of this question, all that can be signified is that the infant, being by its original mental constitution susceptible of the notion of space, thus realizes that notion in its first rudimentary form. It must be admitted, moreover, that such a rudimentary notion of space is as yet purely subjective, that is, a state of the conscious self and not yet regarded as something separate from the thinking being. Metaphysicians are accustomed to say that the notion of space is a necessary condition of human thought. This is the same kind of proposition as that made by physiologists when they say, the germ at the time of conception is endowed with susceptibilities which represent all the realities which it is to exhibit up to the moment of its extinction by death. It is doubtless quite correct so to speak; but in philosophy, as in physiology, what we inquire into is not the existence of preliminary conditions or susceptibilities, but the occasions when susceptibilities are realized. If we wish to explain the nature of memory, it is not enough to say that man remembers because he has a susceptibility of memory, any more than it is knowledge to say that opium procures sleep—"quia habet vim dormitivam." It belongs, then, to metaphysicians and physiologists to determine if the first sensations of the infant, as explained above, be sufficient to suggest to its mind a rudimentary notion of space, and not to rest satisfied with the proposition that the notion of space is an essential condition of human thought. The law of sensation, as above proposed, should be supplemented by the statement, that when an impression extends over the extremities of a number of adjacent nervous filaments, there is an obvious tendency in the mind to regard the resulting compound sensation as possessed of a unity of character; and this seems to be the effect of what physiologists have termed the radiation of sensation[1]—the nervous influence generated at the central extremity of each sentient nervous filament, particularly when the impression is strong, flowing over, so to speak, reciprocally on the several filaments concerned. This same radiation of sensation is the physiological explanation of what Sir William Hamilton has pointed out as a law of sensation in relation to perception, "that above a certain point the stronger the sensation, the weaker the perception; and the distincter the perception, the less obtrusive the sensation."[2] For if the impression made on the peripheral extremities of certain nerves be unusually vehement, then the nervous influence generated at their central extremities in the nervous centre extends beyond its proper limits, and confuses the effect at their peripheral extremities.

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[1] Report of the British Association for 1850. Transactions of Sections, p. 135.

[2] Reid's Writings, with Hamilton's Notes, p. 862.

Metaphysicians have never been able to explain the origin of the notion of motion. Now physiology teaches that man is conscious of every degree of contraction of the muscles of locomotion, as well of those contractions which so freely take place automatically in the infant before volition is established, as of those which volition subsequently directs. A remarkable illustration of the subjective knowledge which man possesses throughout life of the actual state of his muscular system is, that, in the dark he knows the exact attitude of his body; that without the necessity for any examination, the moment the attention is directed to this matter, he is at once aware in the darkest room in what particular position he stands, sits or lies. This knowledge of the muscular system implies that each fibre or set of fibres in a locomotive muscle communicates with the nervous centre by means of a sentient nerve. If, then, the law of sensation above stated be applied here, it is manifest that the local seat of the sensation belonging to each muscular sentient filament will change its place during the contraction of the fibre or set of fibres to which it belongs,—so that the curious inquiry arises, whether the consciousness of the change of the sensation in its seat, during the contraction of such muscular fibres, be not the condition on which the rudimentary notion of motion first arises in the mind of the infant. The tendency of the simple sensations to combine into one compound sensation, representing by unity the entire contraction of a large muscle or set of muscles, may gradually extend the first rudimentary notion so as to render the transition easy to the movement of the members on the body by means of the joints. But this question is far from being as yet in a state for solution within our limits.

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[1] See Müller's Physiol,ogy, by Baly, p. 697.

[2] Ib. p. 880.

The metaphysician will be apt to tell us here, that even if the infant, by an original law of its mental constitution, can distinguish one sensation from another by their difference of local seat, or can even distinguish the form of space occupied by one muscle from that occupied by another, through the aggregate of the sensations arising during the contraction of each, that it will not, therefore, know either of these forms of space otherwise than subjectively, that is, as so many states of self. He will insist, therefore, that the physiological mode of inference from sensation does not solve the great difficulty of metaphysics which is, how the mind, which originally views all the phenomena of consciousness as subjective, or as so many states of self, comes at last to view a certain portion of these phenomena as objective, some, namely, as parts of its own body, and others as things without. To which the physiologist may answer, that he admits the difficulty—for that it does not yet appear on what particular condition the infant, even after it is already subjectively acquainted with every one of its parts and members, comes to know these in an objective sense—that is, how, when with the hand it touches the thigh, it comes to discover through the hand that the thigh is touched, and through the thigh that the hand is in the act of touching it.

In short, as yet the infant's view only represents the place of the hand, when impressions are made on it, as known through the aggregate of the minute sensible areas occupied by the peripheral extremities of the sentient nerves, in which, owing to the impressions made, consciousness has arisen, and the place of the thigh as known, when impressions are made upon it, through the minute sensible areas on which consciousness has there simultaneously arisen. But our physiologist may, nevertheless, maintain that by his mode of investigation some advantages, and those, too, of a metaphysical kind, may be obtained, and that even if it be impossible to discover the precise mode in which the phenomena of consciousness representing in the mind of the infant the parts of its own body and things without, pass from a subjective to an objective character, yet that the nature of the difficulty may become more apparent, and its character and real importance more easily apprehended by mankind at large.

But, in truth, it does not seem a wholly hopeless task to attempt to trace out the condition or conditions on which the kind of knowledge here referred to may pass from a subjective to an objective form. The infant, pure idealist as it is assumed to be previously to this change, is nevertheless as well acquainted with the properties of the matter of its own body, and of the external things with which it has come into contact, as any octogenarian follower of Pyrrho ever could be. It must already be familiar with the distinction of what are always to remain purely mental phenomena, into subjective and objective; for, though the act of memory is always subjective, the sweets of yesterday which it remembers, are surely objective. After this stage, how easy is it to conceive that there may be some simple train of thought determined by which the infant shall recognise the identity, as seen with the eye, of that same hand which as yet it has known only through the nerves with which the hand is supplied, and the difficulty vanishes.

If, however, the infant has succeeded in reaching the objective aspect of its own body, it is already far advanced in the study of matter, and the notion of the external will no longer present any difficulty, since whatever it touches which is not a part of self, affords but one sensation, while, in every case in which a part of self is touched by another part of self, two simultaneous sensations arise, each having its local seat in one of the parts which have come into contact.

Did our limits permit, it would be easy on the supposition that the great difficulty involved in the transition from the mere subjective knowledge of the bodily frame to an objective knowledge of it, were overcome, to trace out, on a like plan, a very complete system of physiological psychology. But it is high time we had devoted more of our attention to the little work which has given occasion to these observations.

Our readers will readily obtain a just idea of the character and style of this little work by such a quotation as the following from the second dialogue, headed "Mind and Matter:"—

"I never," said Eubulus, "find myself left to my own contemplations in a situation such as this without a feeling of wonder at myself and my own existence. Here am I, I mean I, who feel and think, pent up within the narrow dwelling of my own body, yet taking cognizance of things remote in space, not only of those which belong to our own world, but of those in the vast universe around us. Marvellous as this may be, let us wait but for a few hours, and we have what is still more marvellous. By the aid of a tube and a few glasses, I may become acquainted with other objects, suns and worlds, distant from us not only in space, but also in time, which I see not as they now are, but as they were many thousands of years before I myself was in existence. I do not say that such reflections prove more than may be proved in other ways, but they certainly impress my mind more strongly with the conviction that, as a percipient, conscious, and intelligent being, I belong to a mode of existence wholly different from that of the senseless bodies by which I am surrounded, and that (even independently of the evidence afforded by revelation) there is nothing unreasonable in the universal expectation of mankind (so universal, indeed, as to have almost the character of an instinct) that there is something in us which will remain, and be capable of perception and thought, and it may be of pure and high aspirations, when the gross material fabric with which it is now associated has become resolved into its original elements."

"CRITES. I can perfectly enter into the sentiments which you have now expressed. The properties of mind are so wholly different from those of matter, the two are so completely asunder, that they do not admit even of the most distant comparison with each other. I can easily imagine that motion, gravitation, heat, light, electricity, magnetism, chemical attraction, have something in common; that they are (as, indeed, Mr Grove has shown them to be) so far of the same essence as to be convertible into each other; but it is to me wholly inconceivable that any exaltation of the known properties of matter should produce the one conscious indivisible being which I feel myself to be. When the materialist argues that we know nothing of mind except as being dependent on material organization, I turn his argument against himself, and say that the existence of my own mind is the only thing of which I have any actual and indubitable knowledge. I cannot help believing in the existence of an external world. Still the hypothesis of its non-existence implies no contradiction, whereas it is as much a contradiction to doubt the existence of my own mind as it would be to doubt that two and two are equal to four. You must excuse me, however, if I say that it occurred to me yesterday (though I did not notice it at the time) that in one of your remarks, you seemed to identify the functions of the mind with those of the body more than you are disposed to identify them at present. I allude to the comparison which you made of the effect produced by long-continued voluntary effort in the maintenance of muscular contraction, and in the operations of the intellect.

"EUBULUS. When we say that we believe in the independent existence of the percipient and thinking principle, I apprehend that neither you nor I can mean to deny the obvious fact of it having a connection with our bodily organs, by means of which it receives impressions from without, and operates in return on bodies external to itself. This, however, is not peculiar to such humble beings as ourselves. When I contemplate the evidence of intention and design which present themselves everywhere around us, but which, to our limited comprehensions, is more especially manifested in the vegetable and animal creations, I cannot avoid attributing the organization of the universe to an intelligent being, whose power and knowledge are such that it is impossible for me to form any adequate conception of them, any more than I can avoid referring the motions of the planets and stars to the same law of gravitation as that which directs the motions of our own globe. But no one, I apprehend, will maintain that the mind of the Deity depends on a certain construction of brain and nerves; and Dr Priestley, the most philosophical of the advocates of the system of materialism, ventures no further than to say that we have no knowledge on the subject. But, to use the words of Sir Isaac Newton, 'This powerful ever-living agent being in all places, is more able to move the bodies within his boundless uniform sensorium, and thereby to form and reform the parts of the universe, than we are, by our will, to move the parts of our own bodies.' The remainder of the passage from which I have made this quotation, is not without interest, as indicating the view which Newton took of the matter in question:—'And yet we are not to consider the world as the body of God, or the several parts thereof as the parts of God. He is an uniform being, void of organs, members, or parts, and they are his creatures, subordinate to him and subservient to him, and he is no more the soul of them than the soul of man is the soul of the species carried through the organs of sense into the place of its sensation, where it perceives them by its immediate presence, without the intervention of any third thing. Tho organs of sense are not for enabling the soul to perceive the species of things in its sensorium, but only for conveying them thither; and God has no need of any such organs, he being everywhere present to the things themselves.'

" ERGATES. I can perfectly well conceive the existence of mind independently of corporeal organs; but our actual experience of it is only under the last-mentioned condition; and in no other way can it be the proper object of study. It seems to me that the best writers on the philosophy of the mind have erred in considering the mind too abstractedly, not taking sufficiently into the account the physical influences to which it is subjected.

" EUBULUS. There are, however, those who form an exception to this rule; for example, Descartes, Hartley, and that universal genius, Dr Hook. Moreover, Dr Reid's inquiry into the human mind is founded on a critical examination of the several senses; and Dr Berkley's essay on the corporeal function of vision contains the germ of all his metaphysical investigations.

CRITES. The two latter were certainly anything but materialists. The others to whom you allude have been guilty of an error the very opposite to that which I have mentioned, giving as an explanation of mental phenomena that which not only has no foundation in observation and experience, but which is, indeed, no explanation at all. When I learn from Hartley that thought is a vibration of the fibres of the brain; and from Hook, that there is a matter in the brain intended to receive the impressions of sound, which may be compared to the bells and vases which Vitruvius describes as being placed in the ancient theatres; and that thinking is the radiation of the soul from one part of the brain to another, I do not find myself to be a whit wiser than I was before.

"EUBULUS. That may be true. But when Hook states that there are various structures in the brain adapting it for the part which it has to perform in connection with the mental principle,—that there is an organ of memory, for example—I find so many facts which are favourable to this opinion, that I cannot but regard it as more than a mere hypothesis. As to this point, however, Ergates has had greater opportunities than I have had of obtaining information; and I should be well pleased to hear what he has to say on the subject.

"ERGATES. If I comply with your wishes, I must make some small demand on your patience, as, although what I have to say may not be much in substance, it cannot be compressed into a very few words."—(Pp. 35–42.)

The remainder of the dialogue contains an account of the brain represented as a congeries of organs, each possessed of a separate endowment, while these several endowments concur to one purpose; a notice of the physiological researches of Magendie and Flourens indicating, for example, that the cerebellum has for one of its offices to combine the action of the voluntary muscles for the purpose of locomotion, that the vision of each eye depends on the integrity of the opposite upper tubercle in the *corpora quadrigemina*, and that respiration and instinctive actions can only be performed while the *medulla oblongata* is entire; observations on the connection between peculiarities in the propensities and characters of individuals and slight differences in the physical organization of their nervous systems; a discussion of the question, is there an organ of speech in the nervous system, and the commencement of a conversation on memory, which is continued into the next dialogue. In support of the idea that there is in the nervous system an organ of speech, Ergates (who speaks the author's sentiments) refers to two cases which had fallen within his own knowledge of defect of speech in young persons whose hearing and mental faculties in general were complete. One of these cases was that of a boy only five years old when Sir Benjamin first saw him, but at the age of thirteen he had heard of him as still unable to speak, though he wrote beautifully, and was clever in arithmetic; the other was that of a girl eleven years old, who, though she could not speak, could, when desired, readily point out the letters of the alphabet in a book she had never seen before.

These are very interesting cases, and imply at least a defect or derangement of the nerves supplying the organs of articulation; they correspond to the dumbness which has sometimes arisen from the effects of lesion of parts by accidents or otherwise. We hardly think, however, they warrant any conclusion in favour of a special organ in the nervous system for articulation. Articulation, doubtless, implies the concurrent contractions of numerous muscles in almost endless combinations: but speech is acquired by long continued efforts, much in the same way as other complex muscular exertions; for example, the ready command of an accomplished performer on the piano over the movements of his fingers. The acquisition of articulate speech is, however, one of the most surprising of all man's attainments in the combination of complex muscular movements. It is manifestly the result of imitation, but of an imitation in which the eye affords little or none of that assistance so fully afforded in most other cases of acquisition in that manner. The child hears a sound and attempts to imitate it; a rude resemblance to the sound is at last produced, and a perfect imitation is perhaps not attained for years. Were the attempt at articulation instinctive, the resemblance would be complete at the first trials. It is an exercise of volition; but what is the condition on which volition succeeds? Physiology seems already to point certainly to the condition—namely, when the individual already has been conscious of the suite of sensations accompanying the contraction of a combination of muscles, and can recall the remembrance of that consciousness, then volition suffices for its repetition—that is, the remembrance of the sensation accompanying the contraction of a muscle or set of muscles is the condition on which volition can reproduce it. If the ordinary skeleton muscles never contracted except in obedience to volition this condition would be unattainable; but nothing is more certain than that every muscle in the body must have many times contracted, in numerous combinations, automatically—that is, independently of volition, long before the age at which articulate speech begins, and that the sensations attendant on their contractions must have been simultaneously impressed on the memory of the child. The efforts at articulation, then, in a child consist of attempts to throw the muscles concerned into various combinations by the remembrance of the sensations which had accompanied their previously observed contractions; and when, by a happy hit, the just combination is made, the remembrance of the corresponding compound sensation will secure its repetition under the act of volition. The awkward movements made at first by a dancer or a skater to hit the model acts are plainly efforts used to remember the sensations which accompanied their previous nearest approaches to the required combinations.

The conversation on memory leads to the suggestion of ideas by internal causes acting on the brain, by the nerves or through the medium of the blood, and to the striking examples which dreams afford of such influences, and from dreams there is a natural transition to phantoms. Under the head of phantoms, and false perceptions simulating realities, numerous interesting cases are mentioned, some of them new, others well known to those versed in the literature of the supernatural. Such subjects are naturally followed by a discourse on mental derangement, of which, as we might expect, a brief yet just view is taken. We can afford room only for the following short passage, in which an important discussion on criminal responsibility of the insane is summed up:—

"EUBULUS.—Believing as I do, with Crites, that the subject which you are now discussing is one of great importance as it affects society at large, I have listened with much interest to the observations which you have made. You, Crites, have pointed out the necessity of not confounding, as has been sometimes done, mischievous or absurd propensities, however strong, with actual insanity. You, Ergates, have endeavoured to show that there is no broad line, by which the former can always be distinguished from the latter; and I am inclined to agree with both of you. But I also cannot but assent to the opinion of Crites, when he farther stated that the existence of illusions is not in every instance to be regarded as justifying the plea of want of responsibility. Allow me to add, that it is a very great mistake to suppose that this is a question which can be determined only by medical practitioners; any one of plain common sense, who will give it due consideration, is competent to form an opinion on it, and it belongs fully as much to those whose office it is to administer the law as it does to the medical profession."—(Pp. 103, 104.)

In the fourth Dialogue the subject of dreams is again renewed, and much interesting discussion takes place, for which, however, we must refer our readers to the work itself; but we shall extract the conclusion of this Dialogue, premising that throughout the conversation runs on the functions of the brain:—

"CRITES. Before our conversation for this day is concluded, there is one other inquiry which I would make of Ergates. Believing, as I do, that the percipient, conscious, and intelligent mind belongs to a mode of existence wholly different from that of the senseless bodies by which we are surrounded, still I cannot but admit that there must be certain changes taking place in the nervous system in connection with mental processes, some of these being transient in their nature, while others are so far permanent that they may not be effaced during the longest life. Now, with regard to these changes, Ergates has stated that 'their exact nature is a mystery which we have no means of unravelling, and that this is a kind of knowledge as much beyond our reach as that of the structure of the sun, or of the central parts of the earth.' Not disputing the correctness of this statement, yet I see no reason why we might not be able to form some general notion on the subject, and the following questions naturally present themselves to us. Are the changes which the nervous system undergoes simply mechanical? or are they of the same kind as those chemical changes which take place in inorganic matter? or do they rather belong to that class of phenomena which we refer to imponderable agents, such as electricity and magnetism, by virtue of which a piece of sealing-wax rubbed with a silk handkerchief draws light bodies to itself, or a bar of iron becomes endued with the attractive property of a magnet.

ERGATES. Although these subjects have not been hitherto formally discussed, still you may on some points anticipate my answer from observations which I have already made incidentally.

The very little that we actually know may be comprised in a few words.

1. The transmission of impressions from one part of the nervous system to another, or from the nervous system to the muscular and glandular structures, has a nearer resemblance to the effects produced by the imponderable agents to which you have alluded than to anything else. It seems very probable, indeed, that the nervous force is some modification of that force, which produces the phenomena of electricity and magnetism; and you may recollect that I have already ventured to compare the generation of it by the action of the oxygenised blood on the grey substance of the brain and spinal chord, to the production of the electric force by the action of the acid solution on the metallic plates in the cells of a voltaic battery.

2. We know that the solid parts of the body are in a state of perpetual change. There is a constant influx of new materials supplied by the digestive organs, and in other ways, and a corresponding efflux of the old materials by means of the various excretions, especially by that of the kidneys. The brain itself forms no exception to the general rule. We cannot otherwise account for its growth in the early part of life, nor for the alterations in its structure which arise as the consequence of disease, nor for those other changes which occur in extreme old age. The molecules of the brain in a man of twenty years of age are not the same with those which formed the brain of the same individual when he was ten years old, nor with those of which it will be composed when he arrives at the age of fifty years. The mind preserves its identity, but there is no corresponding identity of the corporeal organ with which it is associated; and we may even venture to assert that the brain of to-day is not precisely and in all respects the same with the brain of yesterday, and that it will not be the brain of to-morrow.

3. We cannot suppose that such deposition of new materials and

abstraction of old ones can be effected by mere mechanical means, as you would take one brick from a building and substitute another in its place. The elements of which the nervous system is composed exist in the blood, but they must undergo a new chemical combination before they can be incorporated with it ; and in like manner they must undergo a chemical change of an opposite kind before they can re-enter the current of the circulation. The precise character of these chemical changes we have no means of ascertaining; but whatever it may be, there is reason to believe, that, in proportion as the nervous system is more or less exercised, whether it be in connection with mere corporeal functions, or with mental processes, so do they take place to a greater or less extent. As relating to this subject, it may be observed that the nervous substance is distinguished from all the other tissues (with the exception of the bones) by the very large proportion of phosphorus which enters into its composition, amounting to 1·5 parts in 100, and to as much as one-thirteenth of the solid matter which remains after the evaporation of the water ; and that one result of over-exercise of the nervous system is the elimination of an unusual quantity of salts containing phosphorus by means of the secretion of the kidneys. This fact was first observed by Dr Prout, who has given it as his opinion 'that the phosphorus in organised beings is in some measure connected with nervous tissues and nervous action,' and who in another place refers to 'severe and protracted debilitating passions and excessive fatigue, as the general exciting causes of' what he terms, 'the phosphatic diathesis.'

4. With regard to those more permanent changes in the brain to which Crites has referred as connected with the memory, and what is called the association of ideas, and I may add, with mental habits and dispositions as far as these are dependent on our physical organization, I have nothing to offer beyond what I have expressed already. There is, I apprehend, sufficient evidence that such changes do certainly take place ; but as to their real nature, we not only know nothing, but have no means of obtaining any actual knowledge. The improved microscope of the present day has enabled us to unravel to a considerable extent the minuter tissues of the animal body ; but nevertheless, in pursuing an inquiry such as this, it affords us no assistance. There can be no doubt that there is as much in the animal structures beyond the reach of the microscope, as there is in the vast universe around us beyond the reach of the telescope ; so that, whatever we might thus discover, we may be sure that there is something further still. But let us suppose that it were otherwise, and (assuming the molecular hypothesis to be true) that with more perfect organs of sense, or more perfect instruments, we could trace exactly the changes which take place in the arrangement or aggregation of the ultimate molecules of the brain, I do not see that we should be much advanced in knowledge. The link between the physical and mental phenomena would still be wanting, and it would be as idle to speculate on the nature of the relation between mind and matter, as on the proximate cause of gravitation, or of magnetic attraction and repulsion."—(Pp. 154–160.)

The fifth Dialogue enters on the mental faculties of animals, and the various forms of instinct. On these subjects many most interesting observations are made, which we regret we cannot present to our readers. We shall quote, however, the concluding paragraphs of this Dialogue, in which reference is made to principles analogous to instincts in the constitution of the human mind:—

"Nor must we omit the operation of another cause which mainly contributes to the attainment of that higher degree of civilization in which the sentiment of duty prevails over the more selfish appetites. The disposition of man, even in his most degraded state, to believe in supernatural agencies, is so universal, and so manifestly the result of his peculiar constitution, that we must regard it as having very much of the character of an instinct. As he advances in knowledge, and has leisure for observation and reflection, the perception of the beauty, grandeur, and harmony of the universe, of the evidence of intention and design, and of the adaptation of means to ends in everything around him, and of the large amount of good with the small proportion of evil, which is manifested in the condition of all living creatures, leads him to the knowledge of an intelligent and beneficent Creator, to whom he *may* at any rate be responsible for the right use of the faculties with which he is endowed: and thus the religious sentiment becomes engrafted on the rude instinct of the savage. Thus, man as he exists under the best form of civilization, is made what he is by the operation of various causes. There are his original instincts, without which he could no more have continued to exist than without the action of the heart. There are habits, which, begun in one, and continued in subsequent generations, become confirmed in him, and bear a close resemblance to instincts. These modify and correct each other, and they are all, in a greater or less degree, under the dominion of the intellect. Such is the general view which we must take of his condition; but if we attempt to make a more exact analysis of it, we find the problem too complicated for a satisfactory solution,—the various influences to which he is subject being so intermixed with each other, that it is impossible for us to determine in each particular instance how much of his sentiments and conduct is to be attributed to one of them, and how much to another.

"CRITES.—You have referred to the disposition of human beings to believe in supernatural agencies as partaking of the character of instincts. If you are correct in so doing, it seems to me that you may with equal reason include in the same category our belief in the existence of a material world; our belief that what we remember as having happened, happened in reality; in short, in all that some have intended to describe under the name of innate ideas; and that Buffier, and Reid after him, regarded as primary and fundamental truths; the knowledge of which is forced upon us by our own constitution, and is independent of experience and reasoning. Now, although I do not admit the exactness of the catalogue of these primary truths, which has been furnished to us by the writers whom I have mentioned, and indeed do not doubt that they have included in it some kinds of knowledge which are derived from other sources, yet I do not dispute the correctness of their general views; and indeed it is plain that it has been practically admitted by even the most sceptical of those philosophers who have written on the subject. But are we really justified in regarding such kinds of belief as being of the nature of instinct?

"EUBULUS.—They differ from them in one very essential circumstance. It has been shown that instincts are far from being constant and immutable; as under a change of circumstances certain instincts are lost, so are others generated. Even those which are of the greatest necessity, which seem to be the most constant, may, under certain circumstances, be found to be wanting in an individual on whom they had been fully developed previously. But it is otherwise with those articles of primary belief which are represented as the foundation of all our knowledge. However strange may be the illusions of the lunatic, or however convincing the arguments of the metaphysician, neither the one nor the other can escape from the belief that there is an external world independent of himself, or that what he remembers to have happened did actually occur. Taking these things into consideration, it seems not unreasonable to suppose that this class of convictions has some higher source than that which belongs to mere instincts, and that they are actually inherent in the mental principle itself, and independent of our physical organization."—(Pp. 204–208.)

We wish our author had afforded some further insight into his sentiments on the subject of the primary and fundamental truths to which he has referred, as enumerated (he says) by Buffier and Reid. Distinct notions on these primary and fundamental truths are of vast importance towards just views in philosophy at large, and were never more necessary than at the present moment, when what is called positive philosophy is striving to throw down all that is most noble in the edifice of human knowledge. Some of these fundamental truths cannot be called in question, because their opposites involve a contradiction; but there are other fundamental truths, just as firmly impressed on the human mind as the necessary truths above referred to, the opposites of which, or greater or less deviations from them, do not involve a contradiction, as in the case of necessary truths. Of such truths, which some have termed natural fundamental truths, the most important are the belief in an external world, and in the free agency of self, the belief that every event has a cause, and that there is an exercise of power whenever an event takes place; for by reasoning from these last truths, the existence of an omnipotent Cause is obtained. We wish Sir Benjamin had given us his sentiments on the question so fiercely agitated among psychologists, How far man's idea of power is derived from the volition which he constantly exercises in the production of muscular movements; and whether the proper mode of stating this point is to say that man derives his notion of power from volition, or that his notion of power or cause is intuitive, and that the exercise of volition is merely one of the occasions on which the notion of power or cause is brought home to his mind?

The last dialogue discusses views of human nature, touches on phrenology, on the causes influencing men's characters, on the mind in old age, on Adam Smith's theory of sympathy, and on self-knowledge, duties, and responsibilities. We quote the concluding paragraphs—

> "CRITES. You may add, that whoever would understand the minds of others, and exercise an useful influence over them, must first understand himself. He who forms a wrong estimate of his own capabilities, of his own prejudices, and of the weak points of his own character, measures the characters of others by an erroneous standard. Not only is he in constant danger of undertaking that which he is not qualified, and of neglecting that which he is qualified, to perform, but he is at the mercy of others, who, although they may very probably be inferior to himself in some of the nobler qualities, obtain a dominion over him by studying his defects, and making them subservient to their private purposes.
>
> "EUBULUS. Whatever they may have been otherwise, the priests of the Delphic Oracle were certainly no impostors when they displayed that simple but significant inscription 'Γνωθι σεαυτον' over the portico of the temple of the heathen god. If self-knowledge be important as the first step towards a knowledge of the characters of others, on other grounds it is more important still. Though we may admit, with Ergates, that the mental principle must be of the same essence, under whatever form it exists, still there can be no question as to the vast superiority of the mind of man to that of all the created beings by whom he is surrounded. But in what does that superiority consist? Other animals, and more especially the gregarious, are not without an ample share of the moral sentiments. We see them displayed in the dog, who rejoices in being your companion, who flies to your assistance if you are attacked; in the attachment of the elephant to his keeper who treats him kindly, and in his resentment of injuries; in the roebuck, who pines and dies if separated from his mate; and even in the cat, who, peaceful at other times, turns round on you in anger if you interfere with her kitten. It is not as to these, but as to his intellectual faculties, that there is so vast a difference between man and other animals, that none can be said even to approach him in this respect. But this distinction is not without its price. It imposes on him duties of a higher order, and greater responsibilities. He is required not to yield to the impulse of the moment, but to look to the more remote consequences of what he says and does; and to keep not only his instincts and passions, but even his thoughts, in

subjection to his will. Nor can this be rightly accomplished by any one who does not regard his own powers, his own disposition, and his peculiar moral temperament, influenced as it is by his physical condition and his mode of life, as a fit object of study, even more than anything external to himself. This brings us to other inquiries of the highest interest, involving as they do so much of what is of the greatest importance to ourselves and others; inquiries which have not been neglected by heathen philosophers, but which assume a more exalted character when pursued by those who, guided by the light of revelation, feel that they are answerable to an almighty power for the right use of the faculties with which they are endowed. But on these we have no leisure to enter at present. Whatever may be the value of our discussions, from the arrangements which you, my friends, have made, we must consider them as terminated.

" '*Quæ cum essent dicta, finem fecimus et ambulandi et disputandi.*' " —(Pp. 240–243.)

The volume closes with some excellent explanatory notes. We have thus endeavoured to present to our readers some idea of the contents of Sir Benjamin's work, and we trust we have said enough to induce those who have leisure to read the book itself. Though entitled Psychological Inquiries, the book is sufficiently light and agreeable reading. Indeed, we wish it had been more of a psychological work, though that could not have been without entailing upon the reader a far more laborious task. The general reader need not fear to engage in the perusal of this book; even in its most profound disquisitions it does not task his attention very severely, and the fatigue of reading abstract propositions is carefully relieved by occasional gentle flights of fancy.

ART. III.—*The Elements of Materia Medica and Therapeutics.* By J. PEREIRA, M.D. Third Edition. Vol. II., Part II.

As long as medicine is an art, so long will the works of Claudius Galen, the last, and perhaps the greatest, of the Hippocratian school, be valuable. For the truth is, that almost all our clinical observations and facts are contained in them; and that for the last sixteen centuries we have added very little to the stock of observed occurrences in disease. Even, too, as long as medicine is cultivated as a science, we may perhaps be the better of Galen. His hypotheses, perhaps, if we could rightly dress them in language, would accord well enough with some modern ones. The frame of man and his mental disposition are the same in all ages, and it is only the outside that varies. We wear "broad cloth," and Galen wore a toga, but we are mutually possessed by the same passions, and by like hopes and manners of thought. So too, perhaps, the doctrine of "heat, dryness, moisture, and cold," may be reproduced in modern times.

Galen probably ceased to be an authority, just because he was made too much an authority. An opinion of his was held to be final, and a sentence of his sufficient to put down any innovator or improver. Whoever ventured to impugn the most trifling doctrine of the school, or suggest the most trivial amendment, was at once put down by an *ipse dixit.* The age of resistance to authority came to medicine as elsewhere; and Paracelsus denounced Galenism, and, above all, Galenical or vegetable remedies, for which he proposed to substitute chemical ones.

Although the established Galenical physicians certainly generally employed remedies derived from the vegetable world, and although they as certainly employed persecution against those who used the newly-discovered chemical compounds, it is far from correct to lay down that they employed no mineral remedies, or supposed remedies. The new school, however, who established the efficacy of these newly-discovered compounds, and of whom Paracelsus may perhaps be taken as the head, denied all respect for the Galenical school; and two great revolutions in medicine were made by Paracelsus when he publicly burned the books of Galen, and when he delivered his lectures at Basle in the vernacular. The authority of Galen became ended, or at least suspended, and scarcely any medical man now-a-days takes the trouble even to read him.

And yet modern therapeutics have, for some years past, tended most decidedly to a revived Galenism. Faith in the efficacy of many of the mineral remedies introduced by the alchemists and their successors has now become considerably shaken. The great value of mercurials in syphilis, as announced by Paracelsus, is certainly not now believed in. The "triumphant chariot of antimony," once so vaunted, is scarcely ever used at all; and were Basil Valentine a modern practitioner, he would not kill his brother monks with this substance. On the other hand, some of the more important Galenical drugs have completely held their ground. Without opium, for instance, the treatment of diseased action would be still more difficult than it is, and conium, hyoscyamus, scammony, veratrum, and a host of others, could be very ill dispensed with.

Not only, too, have these vegetable drugs maintained their reputation among the moderns, but the spirit of Galenism has been revived, and increased attention paid to them. Their active principles have been inquired into and separated, and we can now and often advantageously employ, in place of crude opium and veratrum, morphia, and veratria.

But, perhaps, the greatest triumph of modern and improved Galenism is the many compounds of carbon, oxygen, and hydrogen, that are obtained from vegetable substances (essentially either starch or lignine), *artificially,* alcohol, ether, chloroform, acetic acid, tartaric acid, oxalic acid, and, perhaps, we might add, other acids at present little employed. Of these by far the best account that our language contains, or perhaps that any language contains, is to be found in the concluding part of the late Dr Pereira's Materia Medica. This account is too long for us here to analyze, but we may make the following extracts.

" Alcohol combines with certain salts (as the chlorides and nitrates) to form definite compounds which have been termed alcohates, in which the alcohol appears to act as a substitute for the water of crystallization. Alcohol is a solvent of many organic substances, as volatile oil, fixed oil, resin, extractive, most varieties of sugar, many nitrogenous organic acids, the vegetable alkalies, urea, caseine, gliadine, leucine, and osmazome. It prevents the putrefaction of animal substances, and is in consequence extensively employed in the preservation of anatomical preparations. It acts, in part at least, by excluding air (oxygen) and water, the two powerful promoters of putrefaction; for when animal substances are immersed in spirit, this fluid abstracts water from the tissue, which in consequence shrinks up, and thus prevents putrefaction by removing one of the essential conditions to its production, namely, the presence of water. Its attraction for water and its power of coagulating albuminous substances, are properties which probably assist in rendering it an antiseptic. Alcohol and rectified spirit of wine give greater firmness to and whiten the animal tissues. The latter property is objectionable in the preservation of some morbid specimens, as gelatiniform cancer (*cancer gelatiniforme,* or *areolaire* of Cruveilhier—*matière collöide* of Laennec). A mixture of one part rectified spirit and three water will, however, preserve specimens of the last mentioned disease in a transparent condition."—P. 1943.

We may add that it is stated that Faraday has succeeded not in solidifying, but in reducing to the consistence of castor oil, alcohol. This was effected by a cold of −166°, produced by a bath of solid carbonic acid and ether.

The following is, as contrasted with the therapeutical action of creasote, interesting:—

"*Wood Soot* (Fuligo Ligni) was formerly contained in the list of the materia medica of the British pharmacopœias. It is still in use on the Continent, and statements of its efficacy are occasionally met with in the periodicals. It is a mixture of distilled products from the imperfectly burnt wood, and of ashes, or other fixed matters, carried up the chimney by the current of air. It consists of a pyrogenous or empyreumatic resin called pyrotin, combined with acetic acid, which also saturates the bases (potash, lime, and magnesia) of the ashes which are carried up the chimney. Besides these there are small quantities of sesquioxide of iron, silica, and carbon. Acetate of ammonia, chloride of calcium, and sulphate of iron, are also contained in soot. Moreover, there is extractive matter, part of which is insoluble in alcohol. Lastly to these constituents must be added creasote. Formerly soot was esteemed tonic, antispasmodic, and emmenagogue. It is now principally employed as an external remedy, chiefly in ringworm and other analogous eruptions, and obstinate ulcers. It is employed in the form of decoction (prepared by boiling two handfuls of soot in a pint of water for half an hour), and of ointment (composed of a drachm of soot to an ounce of lard). The decoction has been used as an injection in chronic cystitis. The tincture of soot formerly in the London pharmacopœia, consists of wood soot ℥ij. assafœtida, ℥j, and proof spirit ℥xxij. It is sometimes called soot drops, or hysteric mixture, and is prescribed in doses of one or two tea-spoonfuls in hysteria."—P. 2014.

The application of chloroform to surgical and obstetrical pratice is unquestionably the greatest modern therapeutical discovery. We are, however, a little disappointed with the article on chloroform in Dr Pereira. It is not very satisfactory, somewhat meagre, and disfigured with a cut of a complicated and preposterous machine for inhaling it. He mentions, however, what, perhaps, has not been properly attended to, that the sale of chloroform dissolved in rectified spirit under the name of chloric ether is an impropriety. Chloric ether is the name given by chemists to the oily fluid formed by the action of chlorine upon olefiant gas, and which is sometimes called in the books *Dutch liquid*.

As an instance of the extreme minuteness of detail into which Dr Pereira went, may be mentioned his noticing the existence of oxalic acid in the liquor allantoides of the cow. It is this minuteness and accuracy that renders the work so valuable.

ART. IV.—*Introductory Text-Book of Geology.* By DAVID PAGE, F.G.S. Edinb.: Blackwoods. Pp. 136.

We make no apology for noticing in these pages a work upon geology. That science is now necessary to every properly educated medical man, and, indeed, save in the case of a few professed geologists, is almost entirely indebted to medical men for its culture. We may perhaps even go farther, and, at any rate, look forward to the time when geology will have a practical bearing upon medicine. "Give me," said the old philosopher, "the making of the ballads of a country, and who will may form the laws." Perhaps the scientific man may affirm that if he is told the geology of a district, he can even anticipate the productions of the balladmonger. At any rate, it is certain that the natural productions of a country, or a district of a country, determine, to a very great extent, its arts, its material civilization, and hence, in some degree, the habits and manners of its inhabitants. And there is some reason for thinking that the diseases of a country are more or less connected with its physical geography and its geology. The silurian formations are unquestionably favourable to the production of ague. Typhus fever does not seem to be so abundant over the oolite as in many other formations, and the great prevalence of calculous diseases in some localities is a matter of notoriety.

We are, however, wandering from the book before us. It is an excellent introductory text-book on the science of which it treats, and, while extremely intelligible, it avoids the fault that disfigures so many elementary treatises on science, that of rejecting technical expressions, and of substituting in their place ordinary words, that convey little definite meaning. We have only space for the following extract:—

> "Such are the stratified systems composing the crust of the globe; such the types of vegetable and animal life that have successively peopled its surface. In these we find a long gradation of change and progress, not progress from imperfection to perfection, but from humbler to more highly organized forms. From the lowly sea-weeds of the silurian strata, and marsh-plants of the old red sandstone, we rise to the prolific clubmosses, reeds, tree ferns, and gigantic endogens of the coal measures; from these to the palmy cycads and pines of the

oolite, and from these again to the true timber trees of the present era. So also in the animal, the graptolites and trilobites of the silurian seas are succeeded by the curious bone-clad fish of the old red sandstone, these by the sauroid fishes of the coal measures, the sauroid fishes by the gigantic saurians and reptiles of the oolite, the reptiles of the oolite by the huge mammalia of the tertiary epoch, and these in time give place to present species, with man as the crowning form of created existence."—P. 136.

Our readers will see from this how carefully Mr Page avoids the unphilosophical opinion of the possibility of transmutation of species.

ART. V.—*The Cholera in Berwick Street.* By the SENIOR CURATE of St Luke's, Berwick Street. Second Edition. London: Hope and Co. 1854.

This pamphlet is not, perhaps, one of strictly medical or scientific value such as to entitle it to notice at our hands. But it is a striking and graphic, because an accurate and detailed picture, of a portion of the last epidemic in London, drawn by one well qualified to do justice to its terrible realities. It is, moreover, written in the spirit of truth and soberness, and does not seek, though professedly addressed to the public, to disturb it either by superfluous eloquence, or by erratic theories. We have not space for the statements by which the author conveys to his readers a conception of what one of the daily papers truly called " a deadlier field than the Alma." But we cannot withhold from the following passage:—

" The writer does not know whether he will be accused of endeavouring to create alarm. For his own part, he is by no means sure that he does not desire to create alarm. As for panic, having witnessed none in September, he does not expect any in November; neither does he at all hold with those who insult the memory of the dead by assigning fear as the main, and even sole, cause of Cholera. He laments the loss of many—very many—brave men and women, whose stout hearts shrank not from the performance of duties, the discharge of which could bring them no reward but an approving conscience, and the avoidance of which would have entailed on them neither discovery nor disgrace. To the memory of one such noble woman, the writer has already paid his tribute of honourable mention. With heartfelt pleasure he records that the subject of the following brief narrative still lives to smooth the pillow for many an aching head. A woman was watching by the bed—the death-bed, she imagined—of a kind and affectionate husband. Her children were with her, already orphans in her sight. She had passed one long night of sleepless anxiety; another was before her—the shades of evening fast closing upon the silent group—when a gentle tap summoned her to the door to welcome the sympathizing countenance of a stranger. 'Your husband is ill,' said this unknown friend, 'and you sat up last night with him. I will sit by him to-night. Hush!' (motioning to discourage the expressions of gratitude that were forthcoming) 'we must not disturb him!' and then she proceeded at once to the bedside to nurse the sick man as only a woman—and not every woman—can. It is notorious that for this disease by far the best remedy is an indefatigable and skilful nurse; and so we may believe that, under the blessing of God, this kind, good woman had much to do with the favourable turn taken by her patient in the course of the night. 'There, he is better now,' she said in the morning, 'I think you will be able to manage,' and then withdrew as quietly as she came. Two or three times afterwards she presented herself at the door, but only to ask after the object of her tender care and solicitude, and immediately to retire. 'To be sure she escaped,' the advocate of the *fear* theory[1] will point out, 'for she was not afraid;' and assuredly it was no coward that was thus gliding about unscathed by the pestilence. Neither was the wife, to whose aid she came, a coward; but was she not most terribly afraid for her husband? Or will it be maintained that the fear which deserves the name of cowardice has some mysterious affinity for Cholera, irrespective of its weakening the frame? Confident persons talk and write as if they supposed some such strange connection, though, if pressed upon the subject, they would probably say they mean nothing nothing more than that fear weakens the frame. It is certain, however, that the multitude make no such distinctions; and so it comes to pass that the dissemination of this theory is attended with a result directly opposed to the purpose of its authors. A man who is afraid at all, becomes ten times more so when he be believes his fear to be such a powerful predisposing cause of the disease. Tell him anything—tell him everything—but that. For is it not as certain a principle as any in the philosophy of the human heart, that fear is the least voluntary, the least controllable, of all its affections? So convinced is the writer of the folly and cruelty of the loud and boastful talk so prevalent on this subject, that he made a point of assuring the people that fear itself is no otherwise a cause of the malady than so far as it debilitates the system; and he is satisfied that this is the only rational way of deprecating fear upon such occasions.

We have no doubt of the correctness of these impressions, and as little of the justice of the following remarks:—

"With a few additional observations upon this subject of public alarm, the writer will bring his remarks to a close. That there should have been so little of it was the one gratifying feature, amid scenes that in all other respects were ghastly and dismal to the last degree. With scarce an exception, the people stood by one another, in this season of peril and perplexity, with unflinching and admirable courage. Panic there was none; but it was a most trying time, all the more trying by reason of the uncertainty that prevailed at the first as to the area of the pestilence, and its probable duration. What, for instance, could be more appalling than the idea which naturally occurred to every one during the first two days, that the work of devastation must be going on at the same rate all over the metropolis? And then the thought, how long was it to last, forced itself painfully upon every mind. 'If it goes on at this rate,' said a man in Broad Street to the writer on the Saturday, 'there will soon be none left to tell the tale.' Most welcome was the ray of hope which, on the Tuesday morning, beamed upon the anxious gaze of those who in anyway felt themselves responsible for the conduct of the people. And happily they had gained their confidence by not attempting hitherto to make light of the calamity. Therefore, when they affirmed that it was subsiding, their word was not questioned. Had they gone about asserting that none but the intemperate and the craven-hearted were in danger, they would assuredly have found it hard to gain credit for anything else they might say."

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[1] The propounders of this theory will have difficulty to make it square with the small number of deaths in St James's work-house. "Weren't you afraid," said the writer to some of the old paupers, "when they kept bringing in so many to die?" "We just were," was the answer.

True to these principles, the author is well assured that nothing but good can result from the utmost publicity with respect to the ravages inflicted by cholera. He utterly condemns the policy which seeks to spare the feelings of the public by suppressing, or by not investigating, the truth. He desires to have a daily return of the number of deaths always circulated as widely as possible in infected districts, and hopes that "whenever a similar disaster again befals a London parish . . . . some such gallant and intelligent gentlemen as represented the press on the heights of the Alma, will at once repair to the scene, and publish day by day the results of their observations." And far from believing that this would lead to increased panic, he has no doubt that the opposite effect would be produced, and that we should cease to hear of "employers refusing work to the inhabitants, long after the disease had disappeared—as if, too, a coat or a pair of boots could carry it into a shop, when scores of dead and dying failed to infect the hospitals and the workhouse." If these earnest words do not carry conviction to persons in authority it would be useless to attempt to add to their impressiveness by any reasonings of ours; nevertheless we may take the opportunity of remarking upon the mischief produced by the habitual concealment of the early cases of epidemic disease in a locality, as well as by the disinclination to publish the facts connected with its diffusion. One consequence is that the real sources of an epidemic can rarely be traced—and, as a necessary result of this, the axe cannot be laid to the root of the tree. It is only after disease has laid fast hold on society, that we begin our house to house visitations. In our large towns, in particular, we know nothing of the obscure beginnings of pestilence. Yet who can doubt that here, if we could enforce it, " obsta principiis" should be our motto. We speak without reference to disputed points when we say with the author of this pamphlet that, without being alarmists, we are little disposed, in any emergency in which the public interest is involved, to shrink from publicity for fear of the consequences. Those who know best, and can best appreciate, the people of this country, know that the thought of danger, which chills the blood of the coward, and terrifies him into selfishness and supineness, may be so used as to call up the noblest emotions and highest faculties of our nature. The men of Inkermann and of Alma do not show this truth better than those who perform among the poor of our great cities duties like those so nobly fulfilled by the Curate of St Luke's. The confidence he displays in his countrymen is not a false confidence; and we doubt not it will be repaid with interest; for those who can thus trust in human nature must themselves have laid aside superstition and fear, and must have carefully cultivated all those things which, in the times of old, were said to be " hid from the wise and prudent," though " revealed unto babes." We recommend this pamphlet very heartily to our readers.

PART III.

PROGRESS OF MEDICAL SCIENCE.

Report of the Proceedings of the Medico-Chirurgical Society of Edinburgh during the 33d Session, 1853-54.[1]

Meeting 1. *Wednesday, November* 16, 1853. Dr Simpson, President, in the Chair.

1. *Mr Syme* read a communication entitled "On the Pedunculated Exostosis of the Long Bones." The term exostosis literally signified an inordinate growth of bone, but it had come to be applied to a great variety of morbid conditions in which this circumstance happens to exist. It was to a form of exostosis resembling the natural process connected with the origin and insertion of muscles, and of pedunculated form, to which he now wished to direct attention, and it was mainly the object of the communication to explain circumstances in the constitution of this form of exostosis when proceeding from the long bones of the extremities, which were of considerable consequence both as regards diagnosis and treatment. Its most frequent seat was the inner side of the thigh-bone, a little above the condyle, and the neck of the humerus, just below the insertion of the scapular muscles. It possessed a neck, which varied from half an inch to an inch in length, usually placed obliquely in relation to the shaft, and beyond the neck the exostosis expands into an irregularly rounded form, nodulated on the surface, which is covered by a thin incrustation of cartilage presenting a shining pearly lustre, and having no connection with surrounding parts. Mr Syme had been further led to believe, from the careful observation of several cases, some of which were detailed, that a synovial membrane surrounded the exostosis, and was reflected from its neck so as to form a double covering, just as in the joints. In confirmation of this view, a case was mentioned in which the fluctuation of a fluid had occasioned great embarrassment in the diagnosis, and another in which a quantity of fluid had been formed in effecting the removal of a tumour. As regards the treatment, from a consideration of these circumstances, especially the small point of attachment to the bone, removal of the exostosis was most encouraging, and already in several instances had been practised.

Mr Joseph Lister, at Mr Syme's request, detailed the particulars of his examination of such an exostosis; (the one in question was removed by Mr Syme from the os humeri of a young lady, æt. 20). The general conclusion to which the examination of the growth had led was, that it had grown at the surface of cartilage which became converted into cancellated bone by an ordinary process of ossification, in which, however, the stage of calcification of cartilage occupied an unusually conspicuous position.

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[1] In future an abstract of the proceedings of the Society will appear every quarter.—*Ed. Edin. Med. and Surg. Journal.*

Mr Syme's paper led to some discussion, in which Professor Miller, Mr Lister, Drs Bennett and Cobbold, took part.

2. *Dr Dunsmure* exhibited a phosphatic calculus, the nucleus of which consisted of a portion of the patient's trousers or drawers.[1]

Meeting II. *Wednesday, December* 7, 1853. Dr Simpson, President, in the Chair.

1. *Dr William Scott* read "Practical Remarks on the Mineral Waters of Homberg." During a recent residence in Homberg the author had had his attention directed to the effects produced by its celebrated waters in various diseases. He had, in particular, found the waters beneficial in all forms of biliary derangement, especially in dyspepsia consequent on too close devotion to business, in rheumatism and in gout, in chronic catarrhal affections, in certain skin diseases, and in glandular and hemorrhoidal complaints, &c. &c. Dr Scott's paper farther contained an interesting account of the chemical constitution of the Homberg waters, as also of the topography of the town.

2. The Secretary, for *Mr John Traill of Arbroath*, read an account of "Cases illustrating the Contagious Nature of Epidemic Cholera as it has recently appeared in Arbroath." The number of cases was twenty-six including all the cases that had up to that time occurred. Of these fifteen had died, eight recovered, and the remainder were under treatment. Of the twenty-six there were four which were in all probability imported from Dundee, and seven in which no communication could be traced; but in regard to the origin of the disease in all the others, it was the object of the author to prove that it was due to contagion. The details of all the cases, so far as known, were submitted, and in his narrative Mr Traill entered very fully into all those points regarding situation, sewerage, exposure to malarial influences, &c., which are associated with the manner of the propagation of cholera. The author himself had arrived at the opinion that the history of the epidemic at Arbroath illustrated very decidedly the contagious nature of cholera.

A debate on the contagious or non-contagious nature of cholera followed, in which Mr Tait junior of Dunse (who gave an account of the recent occurrence of the disease in that town), Dr Lowe, Dr W. Gairdner, Dr Simpson, Dr Begbie, Dr Bennett, Dr Wood, &c., &c., took part.

Meeting III. *Wednesday, January* 4, 1854. Dr Taylor, Vice-President, in the Chair.

1. *Dr Simpson* communicated observations at considerable length on "Puerperal Arterial Inflammation and Obstruction." No reference to this peculiar disease was to be found in any work on midwifery. The first case which had come under his own observation, now ten years ago, had been brought under the notice of the Obstetrical Society, and an account of it had appeared in their Transactions in 1847. In detailing the symptoms and features of this interesting case, he had, he believed, first suggested the idea that the obstruction of an

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[1] For the details of this case see this Journal for January 1854.

artery might result from the detachment of a vegetation from the cardiac valves. Recently the whole subject of arterial obstruction had received much elucidation from the observations and interesting collection of cases of Dr Kirkes of London. In the puerperal cases, the following causes of the disease might be assigned:—1st, The separation of organized vegetations from the aortic valves; 2d, The escape of recent coagula from the heart into the circulation; 3d, Certain diseased conditions of the blood, or, morbid matters carried along in its current; 4th, Where there might be the laceration of the inner coat of an artery. Dr Simpson had seen a second case last summer with Dr Moir, and cases had been communicated to him by Dr Macfarlane of Glasgow, Dr Lever of Guy's Hospital, and by Dr Burrows of London. In these five cases, the cause of the disease was apparently the same, viz., the separation of organised vegetations from the aortic valves; and, for these reasons: 1*st*, That in all five the aortic valves were the seat of such vegetations; and. 2*d*, because one or more loose bodies similar in appearance to the cardiac vegetations were found in the obstructed arteries. Into the other causes of arterial obstruction, Dr Simpson also entered at some length, and concluded by the expression of a hope that a subject of so much interest should find investigators among the members of the Society.

A conversation followed, in the course of which Dr Christison related the particulars of an interesting case of spontaneous occlusion of an artery, not puerperal, which had recently been observed by himself. Dr Bennett, Dr W. Gairdner, Dr Matthews Duncan, and Dr Moir, also took part.

Meeting IV. *Wednesday, February* 1, 1854. Dr Simpson, President, in the Chair. (Pathological Meeting.)

1. *Dr Simpson* mentioned that since the last meeting of the Society, he had seen a case the symptoms of which were referable to the occlusion of a cerebral artery by a cardiac vegetation.

2. *Dr Reid*, who was introduced by Dr Douglas Maclagan, showed two casts of the dental apparatus of the so-called Aztecs lately exhibited.

3. *Mr Mackenzie* exhibited a preparation illustrative of a case of ununited fracture of the thigh. In its treatment, Mr Mackenzie had followed the method of Diffenbach by introducing ivory pegs; and though union had been effected, the result had been tedious, and amputation had been had recourse to.

4. *Mr Mackenzie* exhibited a preparation in which the obturator artery arose in a common trunk with the epigastric, and encircled the neck of a hernial tumour. It was removed from a patient who had undergone the operation for crural hernia.

5. *Mr Mackenzie* further exhibited a scirrhous testicle; also enlarged mammæ from a male subject in which no true glandular structure existed.

6. *Dr Paterson* showed the heart of a dog, sent to him from Shanghae, in the cavities of which were numerous ascarides.

7. A portion of small intestine 6 inches long, was also exhibited by Dr Paterson, discharged by a young child who had suffered from intus-susception.

8. *Dr W. T. Gairdner* exhibited a series of specimens of degeneration of the liver. Dr G. regarded the waxy liver as the one characteristic of phthisis; the fatty also occurred, but in his experience by no means so frequently. He made observations on both of these morbid conditions. Dr Alison and Dr Sanders also spoke on this subject.

9. *Dr Simpson* showed a uterus which had undergone fatty degeneration, and remarked that this change occurring in the uterus after delivery, was to be regarded as a normal one.

10. *Dr Simpson* showed a fluid containing numerous hydatids, removed by tapping from the peritoneal cavity.

11. *Dr Simpson* further exhibited a drawing illustrative of great enlargement of the superficial lymphatic vessels over the pubis and lower part of the abdomen, arising from obliteration of the deeper vessels. 12. Also two sketches of females affected with a peculiar darkening of the skin of the face. 13. The skeleton of a patient who had died of rickets, and who had undergone the Cæsarian section; and several very contracted pelves were shewn. 14. Preparations of cancer of the body of the uterus where the cervix remained unaffected.

15. *Dr Haldane* exhibited a preparation in which there existed most extensive atheromatous disease of the aorta and iliac arteries, the latter in their internal divisions were plugged by firm coagula, and Dr Haldane believed that this might in the first instance have resulted from the separation of some of the large and loose atheromatous scales.

16. A highly contracted aorta and additional pulmonary valve were also shown by Dr Haldane. In this case no communication existed between the sides of the heart, as is not unfrequently the case when the aorta is much contracted.

Meeting V. *Wednesday, March* 1, 1854. Dr SIMPSON, President, in the Chair.

1. *Mr Mackenzie* made several observations in regard to the operation of excision of the knee joint—reading extracts from letters he had received from the following gentlemen: Mr Page of Carlisle, Dr Keith of Aberdeen, and Mr Butcher of Dublin. The statements contained in these letters appeared to afford a strong corroboration of the views formerly advocated in the Society. At a subsequent part of the evening, Mr Mackenzie exhibited a patient, on whom a year previously the operation had been performed. In this case, the success of the operation appeared complete.

Mr Syme shortly recapitulated the views he had expressed on a former occasion. He maintained that excision could not be substituted for amputation, and though congratulating Mr Mackenzie on the successful issue of the case presented, he questioned the usefulness of the limb that was formed.

Mr Mackenzie believed the limb to be a truly serviceable one. In, nearly every case in which the operation had been recently performed most gratifying results had followed.

2. *Dr Skae* read a paper on the weights and specific gravity of the brain in insanity. The presumption was that in the insane the absolute weight of the brain is increased, but chiefly owing to the increased weight of the cerebellum, and in regard to the specific gravity, that in both white and grey cerebral substance, it was increased. Several Tables were exhibited, and interesting observations in connection with the specific gravity in cases of different forms of mental disease were detailed.

3. *Mr Syme* read a communication on Velo-synthesis or the operation for cleft palate. Mr Syme desired to place on record his objection to the operation which had recently been proposed to remedy the deformity of cleft palate. He regarded the operation as impracticable, and, even if otherwise, capable of being followed by no good result. He expressed his confidence in the old operation.

Mr Spence and Mr Mackenzie took a different view as to the practicability of the operation—the former had seen it performed, and the latter referred to the instances of success which had been published.

Dr Bennett directed attention to an interesting case of heart disease in the clinical ward of the Infirmary.

Dr Simpson made some observations on the recent mortality from measles.

Dr Andrew Wood described an hysterical spasmodic cough which had affected some of the female inmates in an institution for education under his care.

Meeting VI. *Wednesday, April* 5, 1854. Dr SIMPSON, President, in the Chair.

1. *Dr Keiller* read the details of a very interesting case combining the features of hysteria and spurious pregnancy.[1]

2. The Secretary, for *Dr Fraser Thomson* of Perth, read an account of a case of perforation of the bowel from external injury which had recently occurred.

Meeting VII. *Wednesday, April* 19, 1854. Dr SIMPSON, President, in the Chair.

1. *Dr Coldstream* read at considerable length the details of a remarkable case of catalepsy.[2]

2. *Dr W. T. Gairdner* communicated a series of observations in regard to cases of cardiac disease, in which there occurred a systolic murmur at the apex, without deformity of the mitral valve, and on the mechanism of the murmur in such cases, deferring to a future meeting of the Society the demonstration of the anatomy and function of the mitral valve.

Meeting VIII. *Wednesday, May* 3, 1854. Dr JOHN TAYLOR, V.P., in the Chair. (Pathological Meeting.)

1. *Dr W. T. Gairdner* read an abstract of observations in regard to the action of the auriculo-ventricular valves, as a sequel to his communication at last meeting.

2. *Dr Haldane* exhibited a specimen of cancer of the pericardium in a subject affected with cancerous disease of the lung, liver, and bronchial glands.

3. *Dr Haldane* showed a piece of meat, which had been impacted in the pharynx of a man who had perished suddenly.

4. *Dr Haldane* exhibited a specimen of cancer of the pancreas.

5. *Professor Miller* showed a large aneurism of the popliteal artery, removed by amputation. The femoral artery had been ligatured in the usual way, but gangrene had appeared shortly afterwards.

6. *Dr W. T. Gairdner* showed a specimen of dilated bile ducts, from a very aged man; also a stomach showing disease at the cardia, which had not been attended either by vomiting or any other characteristic symptom.

7. *Dr W. T. Gairdner* showed two aneurisms of the aorta.

8. *Dr W. T. Gairdner* presented a section of the muscular fibre of the heart affected by a species of tendinous or fibrous degeneration which prevailed especially in the columnæ carneæ; also two specimens illustrative of a case in which an abscess formed behind the liver, apparently in connexion with the right suprarenal capsule, and opening into the bile ducts, which were much dilated.

9. *Dr Matthews Duncan* showed the larva of an œstrus, probably the common œstrus bovis, which he had extracted from the skin of a girl who had been employed in tending cattle.

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[1] The case is published in the present number.
[2] Published in this Journal for July 1854.

Meeting IX. *Wednesday, June* 7, 1854. Dr Taylor, V.P., in the Chair.

1. *Professor Miller* communicated the particulars of a case of femoral aneurism for which he had placed a ligature on the external iliac artery.

2. *Mr Sidey* gave an account of a case of spontaneous gangrene in a child eight months old. The case was illustrated by coloured casts. The right ear and the entire hairy scalp, the sides of both cheeks, the right thumb, and backs of both hands were affected. The child was convalescent from hooping cough. Death occurred in twelve hours after the appearance of the gangrene of the thumb. Dr Andrew Wood, Dr W. T. Gairdner, Dr Warburton Begbie, and Dr Matthews Duncan referred to cases somewhat similar.

3. *Professor Miller* made some additional remarks on the case of popliteal aneurism (formerly communicated), the preparation of which was exhibited.

Thereafter Dr Taylor, V.P., made a few valedictory remarks, and the Society was adjourned to the third Wednesday of November 1854.

MIDWIFERY.

On Retinitis from Undue Lactation. By William Mackenzie, M.D.

The attention of practitioners is often called to imperfect vision, or what the patient styles *weakness of the eyes*, in women giving suck. The most momentous part of the disease is really a chronic inflammation of the retina. Almost all the other tissues of the eye, however, may be involved, so that it might almost be set down as an ophthalmitis, were it not much more apt to terminate simply in amaurosis, than in any such general disorganization of the organ of vision as occurs in those inflammations to which the name *ophthalmitis* ought to be appropriated. Mr Middlemore describes the disease as affecting chiefly the edges of the eyelids and the conjunctiva, and designates it by the name of *irritable ophthalmia*. Dr Nasse considers it chiefly as an inflammation of the cornea. Dr Ashwell refers to the affection as an amaurosis.

The disease may affect one eye only, but commonly both eyes suffer from it. There is an evident general irritation about the organs of vision. The eyelids are somewhat swollen, and their edges red. The conjunctiva, and especially the palpebral portion of it, is affected with catarrhal inflammation, generally slight, but sufficient to cause adhesion of the eyelids in the morning. There is often some degree of rheumatic sclerotitis, with stinging pain in the eyeball and orbital region. The redness of the conjunctiva or sclerotica is rarely very considerable. The external part of the disease is apt to assume the form of phlyctenular conjunctivitis; then there is intolerance of light; and the cornea becoming involved, presents a small opaque deposition near its centre, which is apt to fall into a state of ulceration. The patient complains of muscæ volitantes, and of such dimness of sight that even the large letters of a title-page cannot be distinguished. The loss of vision often proceeds to the length of disqualifying the patient from knowing one person from another. The pupil is at first contracted, but after a time becomes somewhat dilated and sluggish, while the cornea and sclerotica are found to yield too readily to the pressure of the finger—one of the diagnostic marks of retinitis, upon which most dependence may be placed. The pulse is small

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[1] Published in this Journal for October, 1854.

and quick. The patient complains of debility and general ill health, and is somewhat emaciated. Want of appetite, derangement of the bowels, rigors, flushing in the face, headache, vertigo, a dragging feeling in the back, and a deficiency of milk generally attend the disease.

It is rare to find very young women affected with retinitis from undue lactation. The subjects are generally upwards of thirty, who have had several children, and have nursed them long. When they begin to complain of their eyes, they have almost always been nursing for some months, perhaps for twelve or eighteen; but the disease may also occur soon after they have commenced nursing, or not till they have weaned the child. They often state that they have never been robust, and that their eyes were always weak while nursing, They are often of a scrofulous or rheumatic diathesis, and bear evident marks of an impoverished, as well as irregularly distributed, condition of the blood. Not unfrequently they have suffered, before marriage, from chlorosis, or been weakened by hemorrhagic and leucorrhœal discharges. Fatiguing themselves much with their infant, supporting it almost entirely by their milk, allowing it to suck during the night while they themselves are asleep, not taking a sufficiently varied, digestible, and nutritive diet, but living exclusively upon such articles as tea and bread, and attempting to keep up their strength by alcoholic drinks, are frequent circumstances in their case. Overworking of the eyes in sewing and the like, want of rest during the night, anxiety of mind, and other causes, often conspire with prolonged or undue lactation to produce the disease, Months and even years may elapse, with the best care and treatment, before the symptoms are overcome, and distinct and strong vision restored.

In respect to treatment, the first thing to be done is to give up nursing, The symptoms are often so urgent, that this must be done at once; and without this, other things will be of little or no avail. Notwithstanding the state of general debility, we cannot overlook the signs of local congestion, reaction in the organs of vision, and actual inflammation of the retina. General bleeding is out of the question; but the symptoms are often such as vindicate the taking of blood from the temples by leeches, or by cupping. Counter-irritation, by small blisters behind the ear, to the temple, or on the forehead, will be proper. Pledgets, wrung out of cold water, laid over the eyes, and applied to the forehead and temples, give great relief.

The bowels having been emptied by a laxative, some mild mercurial, such as blue pill or hydrargyrum cum creta, ought to be administered, along with small doses of sulphate of quina, twice or thrice daily, till the gums are touched.

The vision clearing under the above-mentioned remedies, the chief indication will be to restore tone to the system by the cautious use of chalybeates and other tonics. The patient should adopt a mild nutritious diet, partly animal, partly vegetable; but avoid, in general, wine, ale, and spirits. The bowels must be carefully regulated, and perfect exemption from fatigue, hurry, and disturbance should be enjoined. The eyes must be rested, and employed in no trying occupation. Exercise out of doors, and country air, are important auxiliaries in the cure.

The external symptoms which so frequently accompany the disease, require the use of the means commonly recommended for ophthalmia tarsi, phlyctenular conjunctivitis, and corneitis. Many of those symptoms are relieved by the use of a collyrium, containing extract of belladonna, or sulphate of atropia. As the disease subsides, advantage is obtained from dropping occasionally on the eye diluted vinum opii.—*Glasgow Medical Journal*, April 1854.

Anomalous Uterine Pregnancies. By Professor Chiari.

Professor Chiari has related the histories of three cases of pregnancy attending anomalous formation of the uterus:

I. *Pregnancy: uterus bilocular.*—A primipara, æt. 32, of small stature, weak conformation, with great distortion of the spine, was received into the Prague Lying-in-Hospital. The fundus of the uterus reached the scrobiculus cordis; the head presented at the inlet of the pelvis. On examination, a fleshy septum was found dividing the vagina into two halves. By the finger, and by the speculum, the two halves of the os uteri were discovered. The right os was more easily permeable by the uterine sound than the left; and on the right side the head was more distinctly felt. Pregnancy in the right half was diagnosed. On account of the deformity of the pelvis it was determined to induce labour prematurely. The patient was in the 36th week. Scanzoni's method, by irritating the breasts, was first tried, without effect, during six days. Kiwisch's douche was tried: the first time, no result; the second trial was immediately followed by an eclamptic fit, and death in a few minutes. The child was withdrawn, living, by the Cæsarian section, but did not survive. The division of the uterus and vagina into two halves was found complete, down to the vestibulum. The right half was more developed: the placenta was adherent to the fundus of this cavity. The left uterine cavity had thick walls, not greatly differing in appearance from the right; the lining membrane was clothed with a villous, yellow layer, several lines in thickness. The length of this left cavity was nearly the same as that of the right, but its breadth was much less.

II. *Pregnancy: bilocular uterus.*—A primipara came into the hospital, having been delivered of a mature child, by breech-presentation, in the street. The cord had been torn off: the patient was bleeding profusely. The placenta was found adhering to the right side: in attempting to detach it the womb contracted irregularly. When removed, regular contraction followed, leaving the womb the shape of a ball. Peritonitis set in, and the patient died on the fourth day. Autopsy: septic endometris, metrophlebitis and lymphangivitis plexus lumbalis, peritonäitis, sinistra, laceration of the perinæum, œdema of the lungs of both sides, *uterus bilocularis.* The uterine walls were thick, and the cavity divided by a septum, two inches in length, into two halves. The placental-attachment was in the right cavity. The left half, also, was tolerably capacious: its long diameter, two inches; its transverse, nearly the same. (The condition of the lining membrane is not described.)

III. *Pregnancy: one-horned uterus.*—A woman was delivered in the hospital of a seven-month's child, which died of induration of the cellular tissue on the fifth day. The mother was affected with syphilis; she died on the eighth day of septic endometris. The autopsy revealed a one-horned uterus: the left side entirely wanting. The left horn, the left tube, the left ovarium, and the left round ligament, were equally wanting. The uterus was bent over to the right in the abdominal cavity.—*British and Foreign Medical Review*, July 1854.

Prolonged retention of life by Infants who have not breathed.
By Dr Maschka.

The elaborate memoir of Dr Maschka terminates with the following summary:—

1. New-born infants can live without breathing, not merely for half an hour or an hour, but for a very considerably longer period, even in circumstances the most unfavourable.

2. In such cases, there is obviously not only an arrest of blood in the

capillaries of the skin, but the vessels of the different organs are either in a state of permanent contraction, or are filled with a column of stagnant blood.

5. The movements of the heart must gradually become very slow. Here a very important question presents itself. How can there be circulation in the infant without respiration, and therefore without the production of arterial blood? This condition of passive life can only exist when assimilation is at a minimum, and when the oxygen of the maternal blood is consumed very slowly. The maternal blood must in such cases be sufficient to maintain life.

Dr Maschka's essay is contained in the *Vierteljahrschrift für Prakt. Heilkunde*, t. iii., 1854. It contains interesting physiological discussions which will repay perusal.—*Association Journal*, 1854, p. 1105.

Puerperal Convulsions. By Dr CORMACK.

It must be borne in mind, that though the puerperal woman is liable to convulsions from special causes, she is also subject to them from others which act on persons who are not gravid; though from some convulsive attacks—*e.g.* epilepsy—she seems very frequently to be respited, in virtue of her pregnancy. The following arrangement of convulsive affections, with reference to their causes, as they occur both in the puerperal and non-puerperal states, seems to be correct and convenient:—

ALL CONVULSIONS ARISE FROM

1. TOXÆMIA, 2. ANÆMIA, 3. HYPERÆMIA, or	acting directly on the spinal column and medulla oblongata.
4. IRRITATION OF THE EXTREMITIES OF THE NERVES,	acting in a reflex manner on the spinal column and medulla oblongata.

The sources of toxæmia causing convulsions are very various. They may be thus succinctly arranged:—

SOURCES OF TOXÆMIA CAUSING CONVULSIONS.

I. DEFECTIVE DEPURATION OF THE BLOOD.	1. Non-evolution of carbonic acid, &c., by the lungs. 2. Non-elimination of the principles of the bile from the blood. 3. Non-elimination of the principles of the urine from the blood. 4. Non-elimination of urea, &c., by the skin. 5. Non-elimination of accidental effete matters from the blood, by the kidneys and other emunctories.
II. INTRODUCTION OF FOREIGN MATTER INTO THE BLOOD.	1. Inorganic poisons, such as acetate of lead, &c. 2. Organic poisons, such as strychnia, &c. 3. Morbid poisons, such as scarlatina, &c.

While the pregnant woman is not exempt from any of the above causes of toxæmia, she is specially in danger from those comprised under the third and fifth divisions of the first head,—viz., non-elimination of the principles of the urine by the kidney; and non-elimination of accidental effete matters from the blood by the kidneys and other emunctories. While admitting the importance of every emunctory to the puerperal woman, it may still be correctly stated, that all

TOXÆMIC PUERPERAL CONVULSIONS are mainly

1. RENAL, or
2. LOCHIAL;

or they may partake of both, along with other characters. The chief object of the present paper is to point out the importance of the former; but in doing so, I wish explicitly to mention defective elimination from any organ as a cause of more or less toxæmia, and also to recognize non-toxæmic causes of puerperal convulsions.

1. Renal Puerperal Convulsions.—The following diagram shows how pregnancy of itself may cause renal convulsions :—

PREGNANCY
CAUSES

Increased necessity for renal depuration of blood. | A tumour (gravid uterus) causing renal congestion, which

CAUSES
Non-elimination of poisonous excrement from blood :—
TOXÆMIA,
WHICH CAUSES

Action on brain, spinal marrow, and medulla oblongata;
HENCE RESULT

CONVULSIONS.

The existence of organic disease of the kidney greatly augments the risk of renal convulsions during the puerperal state. And, as was formerly stated, the continuance of the pressure of the gravid uterus after the death of the fœtus must be specially apt to induce toxæmia; for if the elements for the nutrition of the fœtus suddenly cease to be required, the maternal blood must for a time be charged with superfluous and foreign matter.

The *prophylaxis* of renal puerperal convulsions must evidently embrace an avoidance of too long continuance in the supine position; an easy corset, giving free play to the lungs, and not pressing back the womb; moderate exercise; regularity and sufficiency of the alvine evacuations, and a good state of the skin. Mental excitement must also be avoided, as it might, even with a moderately poisoned state of the blood, be the immediate cause of convulsions. It is natural to suppose, that, when toxæmia is present, congestion of the nervous centres is more dangerous than when the superabundant blood is healthy.

The *treatment* of renal puerperal convulsions is a subject on which a great deal might be said; but having already exceeded my limits, the leading intentions only are for the present indicated in the following diagram :—

1. Remove pressure from renal vessels, by interdicting supine posture, by unloading the bowels, and (when safe) emptying uterus.
2. Relieve congestion of kidneys by purging, cupping in loins, or general bleeding.

} Reducing Toxæmia.

3. Venesection if apoplexy be threatened.
4. Calmatives to excited nervous system.

} Relieving or arresting Convulsions.

London Journal of Med. 1849.

Prolapsus of the Uterus cured by Retroflexion. By Dr BERNARD SEYFERT.

After showing the insufficiency of the means ordinarily employed against prolapse of the uterus, and going over the principal causes of this troublesome affection, the author records the following observations:—

1. A woman, æt. 30, mother of several children, left her bed soon after her last accouchement, from which there resulted a prolapsus.

The uterus was between the labia, and projected for two inches from the vaginal orifice. The author ordered cold injections several times a-day, and recommended the horizontal position for three weeks.

At the end of this time, the patient could walk easily; a vaginal examination discovered a complete retroflexion.

2. A servant, æt. 19, who had become a mother three years previously, had a prolapsus of the uterus after a painful labour. It was not till nine months afterwards that she consulted a physician. The uterus projected from the vulva to the extent of two and a half inches, and it was, as in the preceding case, much longer and larger than ordinary. The os uteri was covered with a granular ulcer of the size of a five-franc piece, and an acrid liquid flowed from the uterus.

The author first cured the ulcer by cauterization and fomentations of Goulard lotion. One morning he observed that the uterus had reascended spontaneously, and had a tendency to bend backwards. The retroflexion soon became more distinct; the uterus continued to be extremely tender and much pain was produced by the least contact with the uterine sound. But these symptoms yielded to cold injections, and the patient recovered completely.

Founding upon these facts, the author proposes in future to produce retroflexion, when it does not happen of itself, and if the uterus is in a condition favourable for it.—*Gaz. Med. de Paris*, 28 Octobre 1854.

SURGERY.

Report upon the Syphilitic Cases treated at the large Hospital of Vienna, during the years 1852 *and* 1853. By Prof. C. SIGMUND.

This report comprehends, for the two years, 3131 patients, (1502 males and 1625 females.)

Of this number 1118 were affected with gonorrhœa (569 males and 549 females); 1687 had chancres (828 males and 859 females); 127 had no syphilitic affection. We will follow the author in these divisions, in order the better to form an abstract of what is of a nature to interest our readers.

A. *Gonorrhœa* (*Blenorrhagie Syphilitique.*)

I. *In Men.*—1. Simple gonorrhœa has furnished the most cases (225.) The treatment has consisted in cold lotions, local bathing, fomentations, &c.; afterwards injections of the sulphate of zinc (from two to three grains to the ounce of distilled water). These injections have never produced accidents of any nature; it would be wrong therefore to reject them. They dispose so little to the formation of strictures, that these affections have become rare, and it is difficult to meet with them for clinical demonstration. Despite the advantages of this simple treatment, various balsams have also sometimes been administered, principally copaiba and cubebs. When in the space of from five to seven days they produce no effect, their employment is abandoned. The mean duration of treatment has been twenty days.

2. *Inflammation of the Epididymis,* (158 cases; 81 of the right, 65 of the left, and 12 of both sides). Most frequently this affection had no particular origin, if it were not the neglect of suitable precautions during

the presence of the gonorrhœa. The treatment consisted in confinement to bed, diet, saline purgatives, the graduated application of cold in fomentations, and local bathing. Experience has shown the complete inutility of local bleedings, which do not even relieve the pain. At first a moderate temperature is employed, and it is gradually diminished till ice can be made use of; it is necessary then to continue the cold *without interruption* till the pain and swelling subside; then we come back to a fresh temperature, and by degrees to lukewarm fomentations. Cold is the best and most certain calmative. Rarely it cannot be endured; we give them, as an exception, narcotics to a large extent (morphine, henbane, and lupuline). As soon as the inflammation is got rid of, we direct our attention to heal the discharge, if it still exists, by means of injections. The application of strapping, when there is no more inflammation, is useful to favour the absorption of exuded matter.

We have nothing to remark on the subject of buboes, strictures of the urethra, condylomata, and affections of the prostate. This last affection when it passes into the chronic state, is very rebellious and resists most remedial agents.

II. *In Women.*—Urethral and vaginal gonorrhœa are the two most ordinary forms observed in woman. The author treats them by cauterization with the nitrate of silver, when no inflammation is present, and by injections of the sulphate of zinc and alum. For vaginal gonorrhœa in particular, the author recommends above all things attention to cleanliness, then astringent injections, and sometimes a plug (tampon), saturated with the same liquids; this to be renewed four or six times a day.

Uterine blenorrhagia belongs rarely to the category of syphilitic diseases, as the fruitless attempts at inoculation prove. The author treats these affections by injections of cold water, or rendered slightly astringent.

Buboes are comparatively rare in females; they offer nothing worthy of remark.

Inflammation of the mucous follicles of the vagina is rarely a simple affection; it accompanies chancres, gonorrhœa, &c. In three cases it produced recto-vaginal fistula.

The number of patients affected with condylomata has been considerable; they are produced, or kept up by the vaginal discharge, and it is indispensable to heal the latter, if one wishes to extirpate the excrescences in question.

The author mentions several new remedies employed in the treatment of gonorrhœa; the balsam of Gedda, analogous to copaiba; the tincture of savine leaves, recommended in chronic discharges, which ought to be recently prepared; benzoic acid, useful in catarrh of the bladder (doze from a quarter to half a gramme;) lupuline useful in painful micturition, and in cordée (the tenth of a gramme repeated two or three times); the balsams of Peru and Tolu, and turpentine,—the first two rather dear, the latter badly supported; injections of the sulphate of cadmium (from the tenth to the fourth of a gramme, in thirty grammes of water), the acetate and sulphate of copper. All these have rendered good service.

B. *Syphilitic Chancres.*

1. *In Males* (828 cases). In several instances these were developed in unusual situations on the fingers, the toes, the internal surface of the thigh, the umbilicus, the orifice of the urethra. In doubtful cases, recourse was had to inoculation. Cauterization always formed the basis of treatment; (caustic potass combined with caustic lime in the proportion of two to one.) In cases of phymosis the author incised the prepuce, laid bare the ulcers and cauterized them, and thus abridged the duration of the treatment, and diminished the chances of secondary infection.

Buboes (255 cases.) Cold applications proved useful at the period of acute inflammation; blisters were advantageously superseded by the tincture of iodine. In less painful cases, compression was employed by means of plates of lead or a hernia bandage. Mercurial frictions were at the same time made on the inner part of the thigh (half a gramme a day). Since the abscesses have been opened with caustic paste (pot. caust. 2 parts, caust. lime 1 part), gangrenous inflammations have become much less frequent.

Secondary Syphilis (356 cases). Ulcers of the pharynx (85), have been the most frequent form, next the papular (67), the scaly (48), the maculated (47), the pustular, &c., eruptions. The other forms (affections of the bones, tubercular eruptions, cutaneous ulcers, iritis), have been very rare. The diagnosis of secondary syphilis never rests on one sign only; it is deduced from the inspection of the primitive ulcers, of which there are ordinarily still traces, and an attentive examination of the lymphatic glands in different regions of the body.

Mercurial treatment has always shown itself the most efficacious. (Corrosive sublimate, blue ointment, the proto and deuto-iodide, and the red oxide of mercury.) Very little use was made of the iodide of potassium and woody decoctions, but often as adjuvants, bitters, quinine, iron, and cod-liver oil, were prescribed, giving one day the specific, and the next the accessory remedy. The author is very fond of cod-liver oil, which he has given to the extent of nine spoonfuls a day.

2. *Chancres in Females* (859). The unusual places where primitive chancres have been found are the fingers (2), the toes (3), the navel (4), the anus (52), the vaginal portion of the uterus (13). In all these cases there were also chancres on the genital organs. There were 75 cases of bubo, a much less proportion than with the men. The secondary forms were more numerous, and among these we must reckon the large condylomata situated in the neighbourhood of the genitals (195), which were necessarily requiring long-continued treatment. The females also had a greater number of cutaneous ulcers and affections of the bones. Mercurial preparations here, as with the men, have been most advantageous, even in the case of pregnant females. The author remarked that in cases of complete cure, the infant came into the world untainted by syphilis.

The author recommends in the treatment of condylomata, which have resisted other means, an alcoholic solution of the sublimate (one of the sublimate to eight of spirits of wine), with which the condylomata are to be touched two or three times a week, or every day, when they are numerous.

Falling off of the hair sometimes takes place during the course of secondary syphilis: it is an effect of the malady, and not of the remedies employed; it is observed oftener in females than males. The author caused the hair to be cut off, and the head rubbed every evening with a pommade, containing from the sixth to the eighth of tincture of cantharides. The author approves also for hastening the growth of the hair, of croton oil (8 drops to 4 grammes of axunge.)

After some lines devoted to non-syphilitic cases, and those followed by death, the author concludes the work by an article entitled, *Particular Remarks*, in which he speaks of inoculation, the transmission of secondary syphilis, vegetations, the passage of primary chancres into secondary forms, of salivation, and the treatment of syphilis, viewed historically. Under this last head the author gives a summary of 1307 cases treated by him by different methods, and he makes it evident once more that we cannot absolutely give a preference to mercurials, to preparations of iodine, or other medicaments, but that it is necessary to be able to discern the circumstances in which it is most expedient to employ one or other method of treatment.—*Gazette Médicale de Paris*, Décembre 2, 1854.

Extract from the Records of the Boston Society for Medical Improvement. By Wm. W. Morland, M.D., Secretary.

Caries of Elbow-Joint.

The specimen was shown by Mr J. Mason Warren. The patient was a female 19 years old, of a scrofulous habit. Two years ago, she was under medical treatment for some difficulty in the lungs. Eighteen months since, a swelling appeared near the elbow-joint, which gradually increased to a large size, when it was opened, discharging a great quantity of pus. No dead bone could be detected with the probe. The forearm remained for a time fixed in a bent position on the arm, but suddenly, four months ago, in a single night became extended, and has hung in that position entirely immoveable and useless since. Her general health had become much affected by this constant source of irritation and pain. At a consultation held on her case, it was at first decided not to amputate the limb, on account of the affection of the lungs, which was evidently tuberculous; but at a second examination of the case, two months subsequently, it was determined to remove the limb, *especially as the patient herself strongly urged it.* The arm above the swelling was quite small, yet a number of large vessels required ligatures, evidently going to supply the swelling below.

Since the operation, the patient has improved; and now at the end of two weeks, is able to be up and go down stairs; her cough less, and all the functions performed naturally. An examination of the joint disclosed very extensive disease. The cartilages on the head of the bones were entirely destroyed. The fibro-cartilage, on which the radius revolves, had disappeared. The joint was filled with pus, and sinuses extended from it in two or three directions. All the muscles in the neighbourhood had undergone fatty degeneration.—*American Journal of the Medical Sciences*, October 1854.

We have given this case entire, not from any peculiarly interesting features it contains, being merely an ordinary illustration of the by no means uncommon scrofulous disease of the elbow-joint; but for the purpose of showing that a society, instituted for medical improvement, as it did not challenge the recorded malpractice, appears to be unacquainted with one of the most successful modern improvements in surgery, viz., resection of the elbow-joint, evidently the proper procedure in the case in question.

Condition of a Joint after Resection.

Mr Henry Smith exhibited the bones which had been removed from the lower extremity of a boy who had, two years previously, undergone excision of the knee-joint by Mr Jones of Jersey, had entirely recovered from the operation, and had died from disease of the liver, a few weeks ago. A firm bony anchylosis had taken place at the site of operation, between the extremity of the former and the tibia, in nearly a straight position. The patella, also, which had not been removed in the operation, was united to the anterior part of the femur, and its ligament, was still attached to it and to the tibia, so that altogether a very firm, compact mass of bony tissue occupied the situation of the knee-joint. This preparation demonstrated that bony union does take place between the ends of the bones of the resection of the knee, and that a most useful limb may be saved. In the instance of this boy, as also in that of others he had seen and heard of, there was great facility of walking. The patient, from whom this most instructive specimen was removed, was the younger of the two boys who had a year ago been exhibited to the Society, and the Fellows had seen for themselves how well he was able to use the limb. There was at that time some disease of the hip on the other side, and since his death it was discovered that dislocation upwards had occurred. This case was a sufficient answer to the objection that had been raised, of making the limb by the operation " like a flail."

Mr Jones, on being applied to, said that he had no remarks to make, except to urge upon surgeons, in cases of operations for disease of the knee-joint, to save the patella, and not divide the ligament. When these parts were saved, there was less difficulty in walking than when they were removed. In answer to a question, he said, the boy from whom the preparation had been taken, had not suffered from any tubercular disease, but had died from extensive disease of the liver, spleen, and kidneys. Two months before he was quite well, and walked about with ease."—*Report in the Lancet of December* 9, 1854, *of the Proceedings of the Medical Society of London.*

The anatomical preparations, exhibited to the Medical Society of London, appear to have illustrated satisfactorily the possibility of a limb practically useful being obtained by resection of the knee-joint.

We confess, however, to having a difficulty in reconciling the pathological description furnished by Mr Henry Smith with the statement of Mr Jones, that " two months before he was quite well, and walked about with ease;" for we can hardly conceive a more serious obstacle to easy locomotion, than dislocation of the hip on one side, and anchylosis of the knee on the other.

Second Case of Excision of the Knee-joint successfully treated by WM. KEITH, M.D., Senior Surgeon to the Aberdeen Royal Infirmary.

John Keith, æt. 14, was admitted on the 8th of May 1854 into the Royal Infirmary, Aberdeen, under the care of Dr Keith. He was of a scrofulous habit of body, having numerous cicatrices of former abscesses all over his person. The right lower extremity was much wasted, and the knee-joint partially anchylosed, the leg being flexed at a right angle on the thigh, and fixed in that position. The joint was swollen and painful, with fistulous openings on both sides of the ham.

On the 17th of May excision of the joint was practised. A semilunar incision was carried along the lower margin of the patella, according to the plan adopted by the late Dr Richard Mackenzie. The patella was deprived of its cartilage, and the gelatinous synovial membrane dissected from the flap. About an inch and five-eighths was removed from the former, and about three-eighths of an inch from the tibia. The limb was extended in a well-fitting Macintyre's metal fracture-frame; six stitches were inserted, and the wound was dressed with ointment, compresses, and a many-tailed bandage.

On 19th May, the dressings were renewed; the wound was adherent throughout its extent, and there was little discharge.

20th May, he was seized with erysipelas of the face, which terminated by desquamation of the cuticle on the 26th. With the exception of several small abscesses, which from time to time broke out in the neighbourhood of the joint, the case went on satisfactorily, and on 9th September, 115 days after the operation he was dismissed cured.

On 30th October, the 166th day, he walked to the hospital, a distance of nearly a mile, with ease, the limb being all but straight, and the joint stiffly anchylosed. The leg was shorter than its fellow by about two inches and a quarter. The limb had become plump, and the boy florid and healthy.

In his remarks on this case, Dr Keith directs attention to the benefit to be derived from removing as much as possible of the synovial membrane which retards the bony union, and also cutting through the femur and tibia on parallel planes, so that they may afterwards be accurately adapted. The line of section in the femur should be close to the edge of the articulating cartilage of the inner condyle. No portion of the flap should either be pared off or destroyed.—*Monthly Journal of Medical Science*, December 1854.

Mr Butcher on Wounds of Arteries.

The following interesting cases have been published by Mr Butcher in the Dublin Quarterly Journal of Medical Science:—

Case I.—*Wound of the Profunda Artery; Ligature of the Femoral below the margin of Poupart's Ligament, by a transverse incision; arrest of the bleeding.*

Patrick Flattery, æt. 22, a shoemaker, was admitted into Mercer's Hospital on the morning of the 26th February 1854. In a drunken quarrel he had accidentally inflicted on himself a wound in the middle of the right thigh with a shoemaker's knife, which penetrated to the hilt. The blood gushed out in a florid stream, and in a few seconds, syncope supervened. While being conveyed to the hospital he became sensible, and the bleeding which had been arrested, broke out again. On admission, at 2 A.M., he was sensible and able to speak, but indistinctly, his face was blanched and covered with sweat; the action of the heart was feeble, and the pulse scarcely perceptible at the wrist. There being no further bleeding, the edges of the wound were brought together, and a compress applied. At 3 P.M., hemorrhage recurred, and was arrested by a tourniquet at the groin. Mr Butcher then proceeded to examine the wound. It was an inch and a half in extent, its long axis being parallel to that of the limb, situated a little above the centre of the thigh, and so closely related to the fibrous sheath, inclosing the femoral artery and vein, as to lay open a portion of its wall. The wound was dilated to the extent of six inches, and the bleeding point sought for, but it was found to lie so deep in the fleshy thigh, that ligature was impracticable, so he at once decided on tying the femoral below Poupart's ligament. The usual place for ligature of the vessel could not be gained access to in consequence of the tourniquet, which it was not deemed safe to relax.

A transverse incision was carried across the artery at the lower margin of Poupart's ligament, and after some little dissection, the artery exposed and secured. The original wound was plugged with lint, the limb was wrapped in cotton wadding, and various remedies to assist the circulation were employed. The patient, however, sank the next morning at 2 P.M., without any return of the hemorrhage, and when he appeared to be going on favourably.

Examination after death, showed the profunda artery to have been wounded immediately behind the upper margin of the adductor longus tendon. A portion of the coats of the vessel fully three quarters of an inch in extent had been sliced out, and an aperture formed irremediable by nature.

The remarks on this case fully justify the mode of treatment which was adopted. At first no interference was called for, till hemorrhage again occurred, then the legitimate practice of dilating the wound was put in force, and a search for the bleeding points had recourse to; being foiled in this, ligature of the main trunk below Poupart's ligament was effected; and though the case terminated unfavourably, it appears to have done so mainly from the excessive loss of blood which had taken place previous to his admission into the hospital.

Case II.—*Wound of the Posterior Tibial Artery successfully treated by pressure at the site of injury, and compression of the Popliteal and Femoral Arteries; Complication; Traumatic Delirium; Recovery.*

On the 6th of March 1854, at half-past one, A.M., Thomas Ryan, æt. 28, a police constable, was brought to Mercer's Hospital, having sustained several wounds accompanied by severe loss of blood. These had been inflicted by a knife in a struggle with a prisoner. The most serious wound was in the calf of the left leg, at the junction of the middle with the upper third. A large amount of blood had been lost, and when brought to the hospital the bleeding broke out afresh. From the position of the wound and extent of the hemorrhage, there could be no doubt that the posterior tibial artery was injured. The wound was about three quarters of an inch in extent, and parallel to the long axis of the limb. The patient had fainted before being seen by Mr Butcher, and had all the symptoms of excessive loss of blood.

Pressure was applied by means of graduated compresses over the wounds in the calf, and a rolled bandage as a pad over the popliteal artery. The limb was extended on an inclined plane, and the foot raised. In the course of about an hour reaction was fairly established.

On 7th March, as the patient complained of uneasiness from the pad in the ham, an aneurism compressor in the groin was substituted.

On the evening of 8th March he was seized with delirium, which lasted about three days.

On 12th March the compresses were removed, and simple dressing substituted, compression of the femoral however being kept up till the 27th, when it was abandoned.

On 15th April the patient was dismissed cured.

The good effects of not seeking by operation for the wounded artery, unless bleeding was actually going on, was exemplified in this case, for pressure answered all the indications for accomplishing a cure. The advantages also of employing the aneurism compressor deserve to be pointed out, as by this means the current of blood circulating in the main artery is controlled, and time is given to the wounded vessel for the permanent sealing of its divided coats.

Case III.—*Wound of the Ulnar Artery above the Wrist successfully treated by Compression at the Wound, and Pressure over the Brachial Artery.*

James Nichol, æt. 43, a shoemaker, was admitted into Mercer's Hospital on 22d March 1854. He was accidentally wounded by a knife above the wrist, which inflicted a transverse wound in front about an inch in extent, severing two of the flexor tendons and the ulnar artery. Bleeding was violent, and the man almost instantly fainted. On examining the wound only a small superficial artery required ligature, but it was evident from the position of the wound and the great loss of blood, that the ulnar had also been divided. Compresses were applied to the wound and to the brachial artery.

On the 31st the compress was cast off by suppuration.

No further bleeding took place, and the patient was dismissed 6th April.

The successful issue of these cases shows the propriety of not looking for the wounded vessel, unless actual hemorrhage is present; but wounds of arteries in the neck must be excepted, as the hemorrhage is too rapid and profuse, and the wounded vessel is too near the heart to admit of a coagulum forming capable of resisting the column of blood. In these cases, then, diligent search should be instituted, and the vessel secured above and below the wounded point, *when practicable;* if not, the common carotid should be tied.

Case IV.—*Extensive Suicidal Wound of the Throat; Ligature of the right and left Thyroid Arteries; Recovery.*

A young man, æt. 25, was admitted into hospital Oct. 4, 1853. He had cut his throat deeply at the upper part with a razor. The wound extended across the neck between the mastoid muscles; the larynx was freely laid open, the lower part of the thyroid cartilage being sliced downwards. The hemorrhage was rapid, and quickly produced syncope. On Mr Butcher's arrival, the bleeding had ceased; the man was almost pulseless, cold, and clammy. A tube was passed into the stomach, and stimulants injected. Hot sponges were applied to the wound, to induce the divided arteries to bleed. The cut extremities of each of the superior thyroids were at last discovered and secured. No subsequent bleeding took place. On the 8th of November the patient was dismissed cured, with no fistulous orifice to mark the wound of the larynx.

This instance affords an example where another accession of bleeding would most probably have terminated life, therefore the necessity for decided measures; because here, from the locality of the injured vessels, no efficient compress could be applied.

Case V.—*Extensively Comminuted Fracture of both Bones of the Fore Arm, with Excessive Hemorrhage into the Limb from Laceration of the Posterior Interosseal Artery; Recovery.*

James Scarlet, æt. 42, a groom, was admitted into Mercer's Hospital in June 1854. He had sustained a comminuted fracture of the fore-arm, in consequence of a kick from a vicious horse. The ulna was smashed into pieces about three inches below the olecranon, and the radius also comminuted in the same situation. There was a large fluctuating swelling behind, which could only be attributed to the giving way of some deep-seated artery. It was diagnosed to be the posterior interosseal from its proximity to the seat of fracture, and from an impulse being felt both in the radial and ulnar at the wrist. The limb was put up in splints, and pressure by means of a pad applied to the brachial artery. The man was kept entirely in bed till the 30th. With the exception of heat and tension in the broken arm, and latterly inflammation of the elbow joint, all went on well, and on the 15th July he was dismissed. On the 6th of August he was able to resume his occupation.

Case VI.—*Excision of the Index Finger, together with the Metacarpal Bone, performed under the influence of Chloroform; Profuse Hemorrhage on reaction being established; Arrest of the Bleeding by powerful flexure of the injured Limb, together with gentle pressure over the Radial and Ulnar Arteries; Recovery.*

James Porter, æt. 51, an ostler, admitted into Mercer's Hospital, April 1, 1854, with gangrene of the index finger of the right hand, and diffuse inflammation of the surrounding textures. Free incisions checked the progress of the inflammation, and the gangrenous finger was removed while the patient was under the influence of chloroform. One artery, the radialis indicis, was tied. Three hours afterwards, rapid bleeding set in from the numerous vessels, and it was not arrested by the ligature of four of them. Compresses were placed over the ulnar and radial arteries; but this was not sufficient, so Mabgaigne's method of flexing the arm and keeping it elevated was resorted to, which perfectly commanded the hemorrhage. For three days the limb was kept in this position, and then it was given up as being no longer necessary. In the course of a short time the patient was dismissed cured.

Mr Butcher's cases, of which merely a short summary has been given, illustrate very satisfactorily the main object for which he has placed them on record—viz. to show the impropriety, except in special cases, such as wounds of the neck, of endeavouring to find a deep seated wounded artery unless hemorrhage be occurring at the time. As regards the use of the aneurism compressor, we cannot help thinking that the fondness of our professional brethren in the sister isle for this instrument may have in some measure influenced Mr Butcher in its adoption in cases where graduated compresses, skilfully applied to the wounded vessel, were sufficient of themselves to arrest any further hemorrhage.

Paralysis of the Portio Dura in connection with Tertiary Syphilis.

M. Roquette, a practitioner at Nantes, relates an example of this accident, the second which he has met with in the course of three years' experience of syphilitic affections. The first case was that of an old soldier, and was at once correctly interpreted. The second, however, in the person of a captain of a coasting vessel, was in the first instance treated unsuccessfully by a practitioner who, misinformed by the patient as to the venereal antecedents and accompaniments, ascribed the disease to one of its common causes, viz., cold, and treated it by local counter-irritation in front of the ear, purgatives, and leeches to the anus. The treatment of M. Roquette was by large doses (℥i per diem) of iodide of potassium, and a blister behind the ear, dressed with mercurial ointment; the cure was complete within six weeks.—*L'Union Médicale,* tome viii., No. 100.

MATERIA MEDICA.

Injection of Iodine and of Lactate of Iron into the Tissues as an Antidote to the Snake-poison.

Dr Daniel Brainard of Chicago, Illinois, has placed before the Academy of Sciences of Paris the results of some experiments on this subject. The serpent made use of was a small rattlesnake, named Massagua (*Crotalophorus trigeminus*); the bite of which, though not fatal to large animals in most cases, is always so to pigeons, which were the subjects of experiment. We do not think it necessary to give the details of the experiments; the following is a synopsis of the results:—

	No. of cases.	Deaths.	Recoveries.	Mean time between bite and death.
Not treated	10	10	0	0 h. 88 m.
Treated by iodine	10	5	5	2 h. 52 m.
——— lactate of iron	3	1	2	
——— cupping alone	4	3	1	
——— tracheotomy	1	1	0	
——— distilled water	1	0	1	
Doubtful	6			
Total	35 experiments.			

Tracheotomy was tried, in supposed conformity with the views of Marshall Hall, as to its efficacy in convulsive disorders. It was, however, useless. Distilled water was injected in one case; an eschar followed, and the animal recovered. Some cases were left without treatment as comparative experiments. In several of the cases the treatment was supposed to have scarcely had a fair trial, the solutions having been employed too dilute, and in one instance the bite being too deep to be reached by them. Dr Brainard has no doubt of the efficacy of these two solutions; he recommends, however, the iodine in preference, as not only less corrosive, but more efficacious; the solution should contain not less than 5 grains of iodine and 15 of iodide of potass to 1 ounce of water. With this strength there is no danger of a local escharotic effect. A cupping glass should be placed on the part before and again after the application of the iodine, which should be injected into the cellular tissue so as to infiltrate all the parts affected by the poison. The cupping glass should be retained for ten minutes after the injection. If the poison, however, have already begun to produce considerable local swelling, it will be right to use a much weaker solution of the antidote, and to cause it to penetrate the whole limb, or at least the whole of the part swollen.—*Gazette Médicale*, 17th and 24th June, No. 25.

Antiseptic Properties of Iodine.

M. Duroy, a druggist in Paris, submits to the Academy of Medicine certain experiments on iodine, which have been favourably received by that body, although it appears that M. Magendie had previously arrived, by less detailed experiments, at a similar conclusion. M. Duroy had his attention called to the antiseptic properties of iodine by a case under the care of MM. Piorry and Forget, in which this substance was injected into an abscess in connection with caries of the vertebræ. The ioduretted water injected was found on its removal, two hours afterwards, to have lost the colour and odour of the iodine; and a test paper impregnated with starch showed that in fact all the free iodine had disappeared. Further experiments showed that this was owing to its entering into combination with the albuminous elements of the pus; and that a similar combination took place when blood, milk, white of egg, and gluten, were placed in contact with free iodine. Further, it appears that albuminous substances have so strong an affinity for iodine as to withdraw it from a previous combination with starch, and to discharge the blue colour which is the well-known result of this combination. (In the presence of organic substances, therefore, starch cannot be securely used as a test for iodine.) In a watery solution iodine does not, according to M. Duroy, coagulate or otherwise alter the form of the albuminous matters with which it combines, but it possesses the power of arresting putrefactive changes; and, after the lapse of a week, ioduretted milk, blood, albumen, and gluten, were found unchanged, while similar quantities, without any addition of iodine, had undergone notable decomposition.

M. Duroy ascribes some of the therapeutic powers of iodine to its antiseptic properties, and suggests that it might be possibly employed with a good result to neutralize certain organic infections and poisons, such as the miasmatic or ague-poison. The commission of the Academy remark, that the experiments of Brainard and Greene, performed since M. Duroy's researches, have tended to show the neutralization of the rattlesnake poison and of the curare (woorara) by iodine under the form of " extrait de lianes."—*Bulletin de l'Académie*, 15th August 1854.

FORENSIC MEDICINE.

Trial of Francis Forbes, accused of the Murder of Ann Harvey.

A trial took place at Edinburgh before the High Court of Justiciary, on Thursday 9th November 1854, which, on several accounts, merits to be put on record as a contribution to forensic medicine. The jury found for the accused, by the verdict peculiar to this part of the United Kingdom, " Not proven."

On Sunday morning the 7th of May, two agricultural labourers, in service on the farm of West Cults, between three and four miles from Aberdeen, saw on the Aberdeen road, at some distance from the nearest house, a shawl close to the wall of a small plantation, a little from the wall a large quantity of blood, and marks of blood on the wall itself. On looking over the wall the body of a female was seen lying. Both men ran back to the farm and gave the alarm. Other parties came to the spot, and in less than an hour a constable arrived and took charge of the body, which, however, was not moved, nor was the face uncovered till between ten and eleven A.M., when the Sheriff-Substitute of Aberdeen, and the procurator-fiscal, with two medical men, came to the place. The face was covered with the gown, on the removal of which the body was seen lying on the back with the throat cut from ear to ear, the head being in a pool of blood. There were other gashes on the body to which reference will be made hereafter. The ground on the side of the wall next the road, at the time it was seen by the medical men, was trodden down so as to suggest to them that a struggle had taken place there. It seems more probable that this was from the feet of the numerous parties who had visited the spot before their arrival, as the constable did not discover any decided marks of a struggle. On the same side of the wall, there was found a bit of flesh of a round form, three quarters of an inch in diameter; also some torn ribbon and the like. A small sum of money was found in the pocket of the deceased; also some letters, and a valentine with the name of the accused upon it. Near the body was a basket containing articles which appeared to be recent purchases, some of them being marked with the names of shopkeepers in Aberdeen. The body being conveyed to a neighbouring workshop, was examined by the medical men, Drs Ogston and Jamieson of Aberdeen.

From their evidence, it appears that the wound in the neck must have been inflicted with great violence by means of some not very sharp cutting instrument. There were also wounds on the hands and elsewhere on the body, particularly close to the privy parts, but the vagina itself was not injured—there were no marks of violation: there was no fœtus in the womb.

In consequence of the name of Francis Forbes being on the valentine, he was apprehended that same day. When the officer entered the house where he lived, almost a mile nearer Aberdeen than the place of the murder, before being spoken to, but on seeing the constable who was in uniform, he cried, so the constable deposed, " You need not come here, it was not me who did it." None of the inmates of the house, however, when put on examination allowed having heard him use these words. When he was shown the body, he said it was Ann Harvey, and then, as some of the bystanders (but not all of them) deposed, turned away with signs of emotion. There were spots of blood on the clothes of the accused, and on the following day his finger was observed to be inflamed. Evidence was produced that he was subject to bleeding at the nose, and the medical men observed blood in his nostrils as if from that cause. There was a shirt found in the house with one sleeve wet as if newly washed; but it was declared that that shirt did not belong to him, but to a man whose clothes were washed in the house.

It appeared that there was an intimacy between the accused and the deceased, and he admitted in his declaration that there was an understanding of marriage between them. In that declaration he said he had not seen her later than the 26th of April—the national fast-day—ten days before the body was found. The deceased lived about four miles farther from Aberdeen than the accused. He had never visited her at the place where she lived just before her death, at which she had been but for three or four weeks. She had made the woman with whom she lived aware of her intimacy with the deceased, and to another woman she had expressed her fears that owing to that intimacy she was with child. She, also, had admitted to one of these women that some difference had arisen between her and the accused. When she left home to go to Aberdeen, on the evening before she was found dead, she intimated to the person with whom she lived that she would be late, and that she expected the accused would come up the road with her, that is would accompany her about four miles beyond his own house. She left her own house after six on the evening of 6th May, having seven miles to walk to Aberdeen, and she was seen in Aberdeen between eight and nine o'clock, and, at least, as late as twenty minutes to eleven o'clock, and at that time she had a basket, containing purchases, which one of the shopkeepers remarked would be heavy for her to carry. In short, it is certain that the deceased was at Aberdeen till near eleven o'clock, so that she could not reach the place where her body was found till considerably past midnight, the distance being about four miles.

Up to this point all is clear and intelligible—the rest of the evidence is contradictory. The aim of the prosecution was to show that the accused was in company with the deceased in Aberdeen on the evening of the 6th May, and that he was still in her company some time after midnight, when the murder is conjectured to have been committed. Several witnesses deposed, some that they had seen the accused, others that they thought that they had seen him in Aberdeen that Saturday night, and a few that they saw him there in company with the deceased. One witness (Kinnaird) who had worked with the deceased and the accused at harvest the previous year, in the south, and who knew that they were then courting, deposed that he saw Ann Harvey in Aberdeen on the evening of the 6th of May, going down Market Street; that he had some talk with her, and that later in the evening, namely, at a quarter past nine o'clock, he had seen her and the accused walking up Market Street together. Another witness (Mary M'Dougal), who declared that she knew both the accused and the deceased, deposed that she saw them together pass and repass her husband's stable in Aberdeen, a little past eleven o'clock. Another witness, the husband of the preceding, who declared that he knew the accused, but not the deceased, deposed that he saw the accused twice pass his stable with a woman, after eleven o'clock that night. Another witness (Cameron), described as a labourer, who said he knew the accused very well, deposed that he thought he saw him in Aberdeen with a woman that evening before ten o'clock, but that when he was returning from poaching late at night, he is sure he saw him with a woman on the road about 100 yards from the place where the body was found.

In contradiction of these statements as to the accused having been seen in company with the deceased at the time specified, his brother and his brother's wife, with whom he lived, and other parties living in their house or frequenting it, deposed to his having been all the evening at home. His brother's wife accompanied her husband to Aberdeen on the evening of the 6th of May. Before she left her own house, a short time after seven o'clock, the accused had come in from his work; she and her husband, with their son, returned home at eleven o'clock, and found in the house the accused, together with two men and two women, and these parties remained for a considerable time before they dispersed. After the two women had gone away, the accused went out with one of the men, who did not reside in the house; but according to his sister-in-law's testimony, returned in a short time; that he went to bed about one o'clock in the morning, and that she did not fall asleep till two o'clock, and that she heard him as in bed up to that time. The brother of the accused gave the like testimony. The four parties referred to as having been in the house together till near midnight, came forward and deposed that the accused was in the house with them for the various periods during which each stayed. One of these (Mackenzie), who lived at some distance, but who got his clothes washed in the house, deposed that he went to the house in question at eight o'clock, and was there till twelve; that the accused was all the while in the house—that he was shaved that night by the accused—that there was whisky during a part of the time—that the accused went out and walked with him a short way, and then turned. This witness besides deposed that when he went to the house at eight o'clock, he found the accused there, and one of the women above referred to (Ann Thomson), and his nephew (Joseph Forbes)[1]; that soon after he went in, Joseph left; that after Joseph left, Andrew Mennie came in for the newspapers, and soon went away; that after some time widow Mennie (the other woman referred to above), came in and stayed for some time, but went away, as he thinks, before the brother and sister-in-law of the accused returned (this is a mistake); that about ten o'clock, Hepburn, who lodged in the house, (one of the men before referred to), came in; that Ann Thomson, (she was accustomed to pass the day in the same house were the accused lodged, but slept in widow Mennie's), went away shortly before the witness; that as to the shirt found with one sleeve wet, and said by the sister-in-law of the accused and by Ann Thomson to belong to him (Mackenzie), he could only say that it was like his shirt. Widow Mennie in her testimony confirms the account of the accused having been at home during the time she was in the house, namely, from half past nine to past eleven o'clock. Andrew Mennie deposed that he saw the accused at home about twenty minutes before eight, when he went for the newspapers, and that his mother left her own house about nine o'clock. Ann Thomson in her deposition gave the same account of the parties in the house, and of what took place in the house that evening, as the witness Mackenzie. She also deposed that the accused was subject to bleeding at the nose. Hepburn, one of the men referred to above, who lodged in the house, deposed that the accused was in the house when he came in about ten o'clock—that he remained till twelve o'clock, when he went out for a short time with Mackenzie, and that on coming in he eat a herring, and went to bed about the same time with himself, namely, at one o'clock in the morning.

The case is one of great difficulty, whatever view is taken of it. That the woman was barbarously murdered does not admit of a doubt—the idea of suicide is not countenanced by a single particular. It seems certain there was no violation in the case—neither was there any robbery. Her basket of small articles was untouched, none of her wearing apparel was taken away, the small sum 1s. 8d. found in her pocket, was probably all that remained after the purchases she had made.

Suspicion naturally fell on Forbes, as he was known to have an intimacy with the deceased. The letters found in her pocket do not appear to have been produced in Court; but in his judicial declaration before the Sheriff, forming part of the evidence according to the Scottish mode of criminal trial, the accused acknowledged that there was an understanding of marriage between them. Further, it appears from the evidence of one of the female friends of the deceased, that there was a proposal for hastening the marriage, because she thought herself with child, and this it was which led to what the deceased described to one of the female witnesses as the little strife between the accused and her on the evening of the national fast-day, ten days before the body was found, and the last time the accused acknowledged to have seen the deceased. On the evening before the body was found the deceased expressed to many persons the strongest expectation that she was to meet the accused before she returned home. It does not appear on what that confident expectation rested—whether merely because it was his wont to be in Aberdeen on the Saturday evening, or because there was an express appointment for that evening at some part of the road. This confidence on her part affords some countenance to the depositions that he was seen on the road to Aberdeen, and in Aberdeen that evening. But of this in the sequel.

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[1] It appears that this Joseph Forbes, whose age is not mentioned, slept with the accused; he seems to have gone to Aberdeen that evening, and to have returned at the same time with his parents. It is stated in his mother's evidence that he went to bed at the same time with the accused; but he was not produced as a witness.

The blood on the clothes of the accused goes for nothing, since the habitual occurrence of bleeding at the nose is fully established, while the spots of blood are so trifling as compared with what must have fallen on the person who lifted the profusely bleeding body over the wall. It is to be remarked, however, that the shirt found on Sunday morning with one sleeve wet, as if recently washed, is a circumstance inferring considerable suspicion, even though it may not have belonged to the accused, since he might easily have worn it the previous night. But in the absence of any blood-stained weapon distinctly traced to the possession of the accused, the blood goes for little. We take it for certain that the razor with which the accused shaved the witness Mackenzie was inquired after, and satisfactorily accounted for.

With respect to the conflicting evidence on the point whether the accused was in Aberdeen on the night in question or at home, it must be confessed that the testimony in favour of the *alibi* holds together better than that for the opposite state of the fact. It is a clear case that if he was in Aberdeen, the accused is guilty; but does it follow, that if he was not in Aberdeen, he is innocent? It seems possible that his brother was mistaken for him. The witness Kinnaird, who had worked with him at harvest, had his testimony strongly impugned by the witnesses for the defence, as being the result of a grudge against the accused. Cameron, the poacher, who affirmed that he first saw the accused and deceased together in Aberdeen, and then about one in the morning, near the place where the body was found, spoke hesitatingly as to the accused being seen in Aberdeen; but his character seems doubtful. The jury could no nothing else than acquit the accused. But did they do him justice in pronouncing "Not proven" rather than "Not guilty?" The evidence for the alibi holds together so much better than that brought forward by the Crown in support of the accused having been in Aberdeen, that it hardly admits of a doubt that he was at home till midnight or near midnight that night, but here the completeness of the alibi fails,—the accused goes out only for ten minutes it is said—but the proof of this short period of absence is very different from that of the previous alibi. At near eleven o'clock the deceased was in Aberdeen, more than three miles distant from the house of the accused, with a heavy basket to carry,—about midnight she must have been passing near his house. Had he an appointment with her? Did the poacher Cameron speak the truth? If the jury had no suspicions of this kind, the verdict should have been "Not guilty." The account of this trial in the Edinburgh newspapers, for instance in the Caledonian Mercury of November 13th, now before us, is full, and, on the whole, correct, with the exception of some of the distances between the places concerned. The additional particulars contained in our account are from an authentic source.

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PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Rise and Progress of Yellow Fever in Peru.*

By Archibald Smith, M.D.

Some years ago, in 1840 and 1841, I published a series of papers on the diseases of Peru in the *Edinburgh Medical and Surgical Journal.* In an article inserted in No. 143 of that periodical I stated that the yellow fever could not be numbered among the diseases of the country.[1] But since then it has been my lot to have returned to the west coast, and to have not only attended in others, but suffered in my own person this very fever. It will not be irrelevant, therefore, to mark the origin and progress of a disease which, at the time when I supplied the pages of the *Edinburgh Medical and Surgical Journal* with the papers referred to, was practically unknown to the medical profession in Peru,

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[1] This statement was again incidentally repeated by me in the year 1848, in a communication made from Lima to Robley Dunglison, M.D., Jefferson College, Philadelphia, which he was pleased to publish in the Medical Examiner, No. xlvi., New Series. The main object of this communication, however, related not to yellow fever, but was meant to establish the fact that tubercular phthisis pulmonalis is one of the most prevalent diseases of the coast of Peru, and more especially of Lima, the capital; for upon this subject the learned editor of the American edition of the Cyclopedia of Practical Medicine had been led to believe the contrary, and expressed himself accordingly, under the article "*Climate.*"

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[illegible]

stomach, cardialgia, and fretfulness, besides cutaneous efflorescences, without the signs of real inflammation."

He further observes, "but it could not be strictly called bilious, because of its short duration, want of hard and vibratory pulse, of thirst, and dry, furred tongue, &c., and because pure bile was rarely shown in the evacuations."

And to the class of symptoms, which might induce one to rank this fever with the pestilential or malignant form, belonged such as "lesion of the cerebral functions, the tendency to hemorrhages, the termination in abscesses, the prostration of strength, and also the period of convalescence, which was painful and tedious, as compared with the active stage of the disease. This form, however, was tempered by the easy mitigation of symptoms, effected either by art or spontaneously; and the mortality attending it was small, considering the great numbers attacked."

"Some wished to *salute* this epidemic under the name of *Vomito Prieto*, or black vomit, and Yellow fever, without adequate reason; for those who vomited of a dark colour were the fewer in number; the yellow tinge, so essential to that disease, was wanting; and some individuals who had been affected with it at a former period were seized with our epidemic notwithstanding, in contradiction to another of the known properties of yellow fever, which is, its not affecting the same person twice."

At the time when this epidemic prevailed there were not wanting, among the different thinkers on its nature and origin, those who attributed its appearance in Lima to the landing of Spanish troops in Callao:—Peru and Chile having just then entered upon the great revolution for their independence; and the reinforcements who had passed the Isthmus were supposed to have conveyed the germs of the *Chagres* fever. Be this as it may, one thing is certain, that if so introduced, it never rose to the dimensions of that parent stock on the sandy coasts of Peru during the epidemic depicted for our instruction by Dr Paredes, and of which the general type may be learned from the preceding transcripts from his text on this subject. Nevertheless, the reader may bear in mind the physiognomy of this fever of 1818, as expressing many of the features that will be seen common to it with the "peste" of 1852.

The first cases of the Lima fever of this latter name and date were vulgarly attributed to the excessively crowded ship-loads, landed at Callao, of poor and sickly Chinese, who were ill-fed, ill-clothed, and badly cared for on the voyage from their native country. But I, as a practitioner, could not trace any symptoms of this fever to them. These Orientals are introduced into Peru, on an extensive scale, in order to load the guano ships in the Chincha Islands, and also to supply the daily diminishing labourers in domestic and field work of the formerly imported negro race. Others, and probably on better grounds, would trace the origin of this *peste* to the arrival of a swarm of unfortunate German immigrants, who were disembarked at the same sea-port, only distant seven English miles from the capital.[1] These deluded people had touched, on their outward passage, at Rio Janeiro, where the yellow fever then prevailed, and landed in Peru in the commencement of the warm season, towards the close of 1851. During the passage round the Horn the disease might have been kept in abeyance, just as we shall find that the epidemic in Lima and vicinity lay dormant during five months of cool weather each year of its occurrence, but only to revive with vigour and energy in the succeeding summers under the fostering rays of a powerful sun.

In relation to this view of the case, I can only say, that I attended very early in the season a gentleman with the *peste*, as it first sprung up in the city, who resided in the house of the German consul; and about the same time Mr Rodulfo himself (the well-known contractor for the German emigrants to Peru) was laid up with the fever, which he told me resembled very much the Rio disease. After they left Rio, and before they got into the cold temperature of Cape Horn, some of the Germans appear, from report, to have been affected with the same on board. Shortly after they were introduced at Lima, I was called to attend several cases at a large mill-work; the humane Mr F. Pfeiffer's, where they were employed in considerable numbers. A fever broke out among them of an unusually severe character—it was of the nervous type, including the general symptoms of the succeeding *peste*, but in a more concentrated form, and in proportion to the numbers attacked, the deaths were excessive. There was, however, no case of black vomit among them, neither was there in the commencement of the Rio fever; and in the Germans, this malady, on the supposition of its being imported by them from Rio, was modified by the subsequent navigation, and the favourable season of their arrival in Lima. They amounted to about 1000 of all ages, and within a few months of their landing, a vast proportion of the adults died away, both in the city and adjacent agricultural estates. But many of them were attacked by dysentery, agues, and other diseases of the country, which must have in no small measure conduced to the great mortality, Everywhere we saw fair-haired and blue-eyed German orphans thrown upon the charity of humane individuals.

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[1] Lima is about 500 feet above the level of the sea.

In Lima, as was also reported to have been the case in Rio, the first season of this epidemic was comparatively mild, especially among the acclimated population. It seemed to require two or three years of incubation, in order to gather its full strength. It began in a form so moderate and insidious among the mass of the inhabitants, that no one at first suspected the formidable powers of future destruction, that were inherent elements of its progressive nature.

During its first appearance in Rio Janeiro, as I have been informed on the respectable authority of Dr Ravena,—now of Lima, but then of the former city,—the epidemic began in a very mild way, and went on increasing in strength, till the third year, when it became most fatal. It was known by the name of " The Polka," since, like the popular dance of the same title, it appeared to be so much the fashion, that almost every one took a part in it.

But before I lay before the reader my account of the rise and progress of the epidemic fever, which was rife in Lima early in 1852, and attained to the full size of a mortiferous yellow fever in 1854, when I left that long-favoured region for England, I hope to be indulged for the digression, if I here relate the circumstances which attended the introduction of the same fever to Guayaquil, a few years prior to its presenting itself on the banks of the Rimac, or in Lima.

On my homeward passage, in one of the Pacific Steam Company's excellent boats, we had occasion to go up the river Guayaquil, as far as the town of the same name, on its banks, surrounded by jungle, and enveloped in a warm, sultry, and humid atmosphere, swarming with insects. Having just left the scene of black vomit, in a climate so different from this, and apparently incalculably less appropriate for its growth or diffusion, I was naturally led to inquire into the local history of this disastrous malady. I rely especially on the clear information and statements of Mr Luzaraga, on this subject.

The facts are these : A Peruvian, well known to his countrymen, Don Toribio Elias, in company with an American engineer, who was engaged to put up machinery on the cotton estates of Don Domingo Elias, in the district of Ica, crossed the Isthmus of Panama, and arrived in the magnificient bay of the same name, where the schooner Reyna Victoria stood, all ready for sea, and they embarked in her without delay. On the same day that they went on board, *both* were seized with headache and fever, which increased upon them for the eight days that they were at sea before this schooner had made the port of Buena Ventura, on the coast of New Granada, where *both* died. But other individuals besides Mr Elias and the American engineer were also taken ill of the like fever, aboard the Reyna Victoria, of whom some died before they could reach Guayaquil, where, in the month of August, this schooner arrived, with several sick of the fever still on board. It is important to say, that this vessel was considered to have been in a perfectly sanitary state when in the bay of Panama; and, therefore, no doubt remains, that Don Toribio Elias and the American, who came direct from New Orleans, by Chagres to Panama, had gone on board the Reyna Victoria affected with the fever to which, in a few days, they were doomed to be victims. The seeds of this fatal disease they might have imbibed into their systems, either at New Orleans, where the yellow fever was in vigour at the time, or on their tedious way up the river Chagres in the wet season, exposed to the usual vicissitudes in an open or partially-covered canoe.

It should also be mentioned, that when the Reyna Victoria with her unhealthy crew, had cast anchor at Guayaquil, this town was perfectly healthy. She entered the port at the lower outskirts of the population, having one of her masts disabled, and in need of other repairs; in consequence of which, carpenters from the shore were sent on board her. But of *ten* carpenters who were thus employed in refitment, not one of them escaped taking the fever while on board, and as they retired to their respective homes, carried the same to their unsuspecting families. From different and independent sources, I learnt that of these ten carpenters, only one survived this fever, all the others having been carried off in a very few days; but, yet more, nearly all their domestic circles, including wives, mothers, and children, &c., became, with alarming rapidity, victims to the same direful visitation, which fell upon them like a thunderbolt.

The unlucky schooner was, moreover, dirty; and her ballast—which consisted of foul mud—said to have been taken in at the always unhealthy, wet and sultry, Buena Ventura, was thrown out at Guayaquil. But on going down the river and to sea again, notwithstanding the great pains taken in cleaning and repairing her, the same fever soon recurred on board; so that she had to put back into the river once more, where, as a measure of police, she was altogether abandoned by her crew, sunk opposite to the island of Puna, and kept a length of time under water for the purpose of extinguishing the remains of febrile poison supposed to be attached to her.

Thus ends the account I received, upon the most satisfactory authority, of the history of the recent introduction of the yellow fever into the republic of Ecuador, which in its seaport, Guayaquil, raged during the years 1842–45; always suffering intermissions of vigour during the cooler, with corresponding activity in the warmer, months. And it is to be observed, that the hot and humid climate along the littoral and thickly-wooded plains of Ecuador afforded the first implantation of this malignant fever a congenial hotbed for its ready growth.

The poison when once admitted proved rapid as well as deadly in its action; it appeared at once in its most concentrated form—passing frequently in a few hours from its commencement to black vomit, sinking, and death. It spread like a consuming fire from house to house, and from street to street; having first broken out around the dwellings of the unfortunate ship-carpenters by the anchorage.

Among the medical practitioners at that ominous time in Guayaquil, was an Irish physician of the name of O'Neil, looked up to for his professional talents and benevolence, but at length he declared his belief to be that the more he attempted to cure his patients of this epidemic, the less good seemed to come of *his* exertions. The individual whose practice was looked upon as the most successful was a humane friar, who was at all hours found at his post of duty aiding the sick and consoling the dying. He allowed only for drink lemonades, cold iced water, tamarind water, or the juice of the pomegranate, passion fruit, or pine apple in water; all of them remedies indigenous to the climate, and such as nature seemed to proportion as suitable for the diseases incident to the locality. As nourishment he permitted rare farinaceous spoon food, gradually improved by a little chicken soup, according as the stomach would bear it.

From Guayaquil we steamed in five days to the island of Taboga off Panama. Here, too, I had further accounts of the yellow fever visit to that secluded spot; which in its evergreen luxuriance, variety of fruit-bearing trees, pine-apple fields, and sweet balmy air, may be looked upon as a little garden of Eden. It has a village built among shady rows of lofty palms, with mango and tamarind groves. It is watered by mountain streams which rush down the wooded cliffs, presenting in their course many pretty falls with corresponding pools, or basins, worn in the rock, which are used as baths of running water. It used to be a garden of refuge to the people of Panama in time of epidemics on the Isthmus. From yellow fever it was reputed exempt; yet, about eighteen months before our arrival there, in May last, it experienced both that disease and cholera. The origin of these diseases in Tabogo seems to have been simultaneous, and accounted for in the same manner.

It happened that American troops, destined for California, were landed on the Isthmus of Panama, over a great extent of which they traversed on foot, in warm uniforms. This is a journey which the native negro or bronzed muleteer performs, when on foot, either naked, or only in a shirt, on account of the oppressive heat. Even in the comparative cool of night, a thin sheet over and another under on a canvass stretcher, is all the bedding usually required or allowed in the best hotels at Panama. The reader, from this fact, will easily conceive what a fatiguing journey the American soldiers must have had in their heavy clothing. Their thirst on the march was naturally much excited by sultry heat and profuse perspiration; and to appease it, unripe fruit or bad water would be taken, as the one or the other came in their way. They arrived at Panama very much exhausted, and were embarked and conveyed outside the bay to Taboga, where the captain of the transport soon found that his voyage to California was arrested by the sudden appearance among the troops of yellow fever, with its fatal attendant black vomit. The captain quietly shifted his position to another but lonely isle in the offing, where, unobserved, he buried his dead, sometimes, as I was told, to the number of twenty or thirty in one day.

But though the affected vessel was removed from Taboga, still the communication was sufficient to have engrafted on the poor people of that rendezvous of the north and south Pacific steamers, both cholera and yellow fever at the same time. It was probably owing to the number of troops crowded into the same vessel, that yellow fever and its associate cholera had so suddenly assumed the fatal type.

The ordinary bilious remittent fever of the Isthmus rarely takes on this aggravated form, and scarcely ever among the acclimated natives. I have seen it break out in New York, in a passenger from aboard one of the American steam vessels, ten days after he had left that stagnant marsh called Colon or Aspinwal, the terminus to the Isthmus Railway on the Atlantic. I have also, on different occasions, seen individual cases of the same fever imported into Lima by passengers from the steamers; but only in a very few instances have I known of such cases terminating with decided symptoms of yellow fever, and such exceptional cases of this nature as I have seen or had notice of, did not give rise to further attacks of the same nature. From so few instances it would not perhaps be legitimate to infer that the Chagres remittent is not contagious, but if the history of the yellow fever of 1842–45 in Guayaquil be duly considered it would appear that under certain favourable circumstances it may become so. Though in offering to arrive at any conclusive judgment on this head, from the data furnished in either of the above accounts, whether of the Taboga or Reyna Victoria details, we cannot confine ourselves to the contemplation of a merely vegetable miasmatic origin, when we have great reason to believe that new elements of a more pestilential nature, under the peculiar circumstances of crowded berths and a tropical atmosphere, might be generated in, and exhaled from, the living bodies of the sick.

Having premised so much with regard to the local history of the yellow fever on the shores of the Pacific, it is now my purpose to lay before my reader a separate and independent account of the rise and development of the same in Lima, as it occurred during three successive years. Should repetition be observed, I must say that on this plan of exhibition repetition cannot be avoided. The epidemic began in a simple form; and though many of the prominent symptoms were carried on from one year to another, yet some fell out of sight, and others came forward into view, until the disease was fully matured. I therefore hold by the order of nature, and let each step in the progress of the disease be marked as I observed and noted down in the order of succession. And that nothing may be kept back from the reader in any way conducive to give him a full idea of the circumstances under which a disease so new in Peru found a beginning, I will just notice, that, in the winter months of July and August of the year 1851, there prevailed in Lima a catarrhal influenza, which proved fatal to many, especially the aged. It left the constitutions of no small number of those who had recovered from it a good deal enfeebled, and thereby predisposed to further disease; but, as far as I am competent to judge in this matter, it had no further relation to the

Epidemic of 1852.—In the month of March of this year, the epidemic, or "*peste*," as it was then called, attained its seasonal height of activity and universality. Throughout the whole of January and the early days of February, my fever patients were mostly Germans, to whose condition I have already called the reader's attention. It was not until the middle of February that cases became so numerous in the general population as to call the public attention; but by the end of this month it became so diffused as to have merited the name of pest or "*peste.*"

Its ordinary symptoms of invasion were pains in the limbs and loins, with headache or a feeling of tension across the brow, eyes injected and face flushed; tongue whitish and furred, with anorexia and flatulency; skin dry and hot, even when the pulse ranged about 80°, and it rarely exceeded 90° Fahrenheit in the febrile stage. In other examples the invasion was more severe, entering with chilliness, nausea, bruised sensations in the limbs and loins, together with much nervous agitation and headache, insomnolency, or disturbed slumber, with illusory, vague ideas. In young persons of tense fibre, just arrived from the cold or more temperate climates of the inland districts, the arterial action was sometimes so strong as at first sight to appear to indicate general depletion. But whenever bleeding had been practised under such delusive appearances, the pulse fell with unexpected rapidity, and the blood being allowed to cool, was not able to bear its own weight when transfixed by a bit of stick, on account of its extreme want of tenacity; besides, its colour was unusually black.

The muscular pains in the back and loins, &c., were constant, in severe cases, for the first three or four days, and continued to be so until the secretions in general became free, and the febrile state had terminated in sweat. This happy solution of the case usually came to pass about the fourth or fifth day, and was often preceded by an attack of epistaxis especially in the Indian race. This fever was attended with little or no thirst, and with the acclimated very moderately-increased action of the heart and arteries, though with a remarkably augmented evolution of dermal heat. There was a peculiar sensitiveness to chill on the slightest exposure of the patient's extremities from under the bed-clothes, notwithstanding the atmospheric heat of the place was above 80° Fahrenheit in the open air and in the shade; and with all these signs there were frequent alternate flushes, and slight, partial, transient perspirations about the breast and head, while the body was for the most part dry and hot. Thus things went on until about the fourth or fifth day, when, as I have mentioned, the febrile symptoms took their departure, with a critical flow of blood from one or both nostrils, or with the appearance of a roseolous eruption, either singly or in connection with some hemorrhagic discharge, and a free perspiration, of the most sickening smell, even to the patient all but intolerable. The transition from fever to apyrexy was often rapid and unexpected, and it sometimes took place not without alarm,—as, for example, in awakening from sleep with a feeble voice and pulse, cold perspiration and cramps, nausea and retching. The roseolous eruption often lasted for many days after the entire disappearance of the fever, as did also the intestinal or uterine hemorrhage when superinduced; for a still longer period the gums were observed to continue spongy, and bleeding occasionally, while there was a propensity to sanguineous exudation from the fauces, which continued at times in an irritable and congested state for several days.

It was uniformly observed, that women who were so unfortunate as to have been overtaken by this epidemic while in the puerperal state, or even when it supervened on the usual catamenial period, were almost sure when the fever took the turn, no matter how moderate in its primary symptoms, to suffer immediate, and sometimes long-protracted, uterine, vaginal, or intestinal hemorrhagic discharges, of a deep black colour, and most offensive smell.

The common cases of this "peste" ran their course in about eight days, leaving the body extremely listless, with very frequently neuralgic pains in the elbows and fingers, &c., recurring at uncertain periods for weeks or months after. Another curious concomitant was so often observed as to deserve to be noticed: the nails, towards their roots, presented elevated transverse ridges, with corresponding indentations, which only disappeared gradually as the nails outgrew the marks. I have also seen similar marks on the growing teeth of young children who have been affected with this fever; but the epidemic itself was not so severe among children as adults.

A great degree of loathing of food usually subsisted for many days during convalescence, and almost all who passed through the fever complained of a morbid taste in the mouth,—in some sweet, in others bitter, or quite salt.

In course of attendance on the sick, I assisted a few who experienced, and had a lively recollection of, the epidemic of 1818, and said it much resembled that which I have now portrayed. In both cases the succeeding languor and debility were very great as compared with the ephemeral and mild nature of the fever itself. The reader will not fail to discover other points of resemblance; and perhaps the catarrhal influenza of the preceding winter, associated with this epidemic of summer, may to some minds be a sequence not unlike (though separated by wider links) to that closer connection betwixt the first catarrhal period and the subsequent development of the fever of 1818, as already referred to in the extracts from Dr Paredes.

The *treatment* adopted in the "peste" now under consideration was very simple.

In the first stage, sedative cooling lotions were generally applied to the forehead, hot water impregnated with salt and mustard to the feet; and, according to the usage of the country, alvine evacuations were solicited by emollient and gently purgative enemata, which the physician usually found administered on his arrival; and it was again well looked to that the bowels, naturally torpid in the febrile period, should be kept in proper order. The general warm bath, and diluent subacid drinks, served to facilitate perspiration, and relieve the impeded freedom of action in the cutaneous capillaries, which appeared to be in a special manner the peculiar seat of morbid heat and irritability. The same object was also promoted by anodynes, such as morphia, hyoscyamus, or aconite, in combination with tartar-emetic in fractional doses; while these remedies were attended with the further advantage that they soothed muscular pains, and conduced to repose or sleep. The morbid, and especially the bitter taste of the mouth was greatly relieved by drinking from time to time a little of the infusion of bitter orange-peel, or fresh lemon rind. The Germans usually drank chamomile-tea, which appeared to be their favourite beverage in all their ailments.

At the commencement of the epidemic, though the gastric system was deranged, there were no decidedly bilious symptoms attending it; but as the hot season advanced, the invasion was occasionally accompanied with a slight bilious appearance in the matter rejected from the stomach, and in such cases full vomiting produced by ipecacuanha or tartar-emetic was a favourite treatment in the hands of several practitioners. In my own practice I did not find such powerful revulsion needed, for a few glassfuls of warm water alone, or with a tea-spoonful of mustard-powder, appeared to clear out the stomach sufficiently; and to quiet gastric irritability, as well as to temper the nervous excitement attending reaction. I made very free use of beverages, consisting of fresh lemon-juice or citric acid, with a proportional quantity of the carbonate of soda, suspended or dissolved in gum-water, slightly sweetened with an aromatic syrup.

In the second stage, when the powers of life became rapidly and greatly depressed, warm cordial drinks, wine, hot chicken soups, dry friction to the skin, stimulant embrocations and sinapisms, hot water to the hands and feet, were resorted to as the case required, or the opportunity offered, in order to excite reaction, which secured the restoration of the patient.

The fatal cases were extremely few, though the fever had overrun all Lima, with hardly the exception of a house or family, in a city of large population; which during six years of prosperity and peace, under General Castilla's presidentship, was so filled up from all parts, that it is now estimated at one hundred thousand inhabitants—nearly double what it was at the close of the war of independence.

In the above account of the treatment, I omitted to say that very many natives recovered favourably under the use of the native chuno, a substitute for arrow-root, as food, with the free use of cooling sub-acid drinks with ice, which proved very grateful to the patient, and favoured the early appearance of perspiration, the surest antidote to the general restlessness and burning heat of skin, which were such distressing symptoms in the first or fever stage. But, on the other hand, as the weather became gradually cooler towards the latter end of April and the whole of May, the slight bilious tendency of the disease, observed in the height of the solar heat of summer, disappeared altogether, and then the ice treatment was abandoned by the natives themselves.

I have only farther to add, in reference to this year's epidemic, that it took its leave towards the end of July, when the Lima winter, at a temperature of about 60° Fahr., damp and drizzly, had fairly set in; but while it prevailed, during a period of five months, the profession had very few other complaints to attend to, so completely did it seem to absorb all the usual diseases of the season.

Epidemic of 1853.—From the end of July 1852 to February 1853, Lima enjoyed perfect immunity from the so-called "peste" of the prior year. In March, the same disease manifested itself in a somewhat less general, but more aggravated form, than in the corresponding month of the preceding season, and a large proportion of those attacked the previous year were again affected in this; but the scarlet eruption seldom was seen, though the former year it was so general a symptom, especially during the transition from the first to the second period. This year's epidemic presented very clearly two stages; one of excitement, and another of depression, of the vital powers. The early symptoms of the invasion were often in the form of a simple fever, such as usually attends a severe cold, with headache, flushed face, and bruised sensations in limbs and loins, &c. But it is a remarkable fact, that the symptoms of the first stage, in the mild, as well as in the more severe forms, were so much alike, that no one could predict the conclusion from the beginning. Thus, it continually happened, that of two in the same family or apartment affected, to all appearance, by the same disorder, and with the same mildness of initiatory symptoms, the one in a few days would find himself convalescent, whilst the other, instead of undergoing this easy transition from moderate fever to perfect recovery, would almost instantaneously, and without any known peculiarity of cause or circumstances, fall into a state of collapse, in which the poor patient sunk at once from the possession of ordinary muscular power to a state of helplessness. This was particularly the course of events where the case was to end fatally, whether with or without black vomit.

In other instances, cases very alarming in the first attack often terminated in the most favourable manner on the fourth or fifth day by a general warm sweat, leaving comparatively little weakness or lassitude behind.

Now and then there were instances of premonitory indisposition, such as stiffness in the limbs, want of appetite, and disinclination for exertion, whether bodily or mentally, for some days previous to a formal attack. For the most part, however, the invasion was made without premonitory symptoms. It would, for example, seize one in the street, with a cold chill, hands and fingers benumbed, and often of a purple hue; symptoms soon followed by muscular pains, rigors, anxiety, nausea, and vomiting. In comparatively rare cases, a burning or painful sensation was experienced at the region of the stomach, and also in the mouth and fauces, with extreme gastric irritability—every thing being rejected almost as soon as swallowed; the matter ejected consisted usually of the ingesta, and rarely with any traces of bile. The face became flushed, and a deep-seated pain with tension was experienced over the orbits, the eye was more or less red and glistening. The tongue had commonly a whitish aspect, but with red edges; at other times its surface was clean and natural-looking, whilst towards the point it was of a bright red, or to appearance raw, like under-done meat. The pulse, during the first few hours of reaction was, sometimes strong, full, and upwards of 120 per minute, and shortly after fell to about 90 or 100, being soft and yielding under the pressure of the finger.

Among the natives of the coast the pulse frequently did not exceed 90 from the beginning of the fever, when all the preceding symptoms though present, were proportionately mild, and the whole course of such favourable cases was sometimes terminated in from thirty to sixty hours. But in other forms of the epidemic, the fever continued for four or five days and nights consecutively, with muscular pains, general restlessness, hot skin, with transient perspiration about the neck and head. In more urgent cases still, there went and came flushes of heat with suddenly oppressed breathing—the respiration being at one time slow with deep involuntary sighing, and again hurried with panting, up to 96 per minute; and yet the pulse bore no natural proportion to this rapidity of respiration—remaining on these occasions about 100, being soft and small.

Little or no thirst was experienced; excretions slow; urine high-coloured; a slight yellow tinge in the colour of the skin; the surface of the body remarkably susceptible of cold on the least exposure from under the bed-covering, of which an unusual quantity was required. And with all this propensity to chill, it was a most prominent fact—as it also was last year—that the skin of the patient felt so very hot to the physician's touch, that he could not avoid considering the animal heat evolved quite in disproportion to the small and bland circulation perceived by the pulse. Besides these pathognomonic indications of more than ordinary cutaneous nervous sensitiveness and capillary activity, the skin itself, in severe cases, was for several days singularly tender to the touch or pressure, while tingling sensations, compared to electric currents, seemed to shoot along the nerves, until lost at the points of the fingers and toes.

Not unfrequently on the third or fourth day of such cases as now portrayed, there was a *lull.* The patient felt tranquil, smoked a cigar, and probably took, or at least asked for, some nourishment, all food being loathed in the febrile period. Under these apparently improving conditions he would fall asleep, but after a lapse of a few hours awake bathed in a cold sweat, with palid countenance, sunken voice, feeble vacillating pulse, exhaustion of vital forces in general, sickness, nausea or retching, in a word, *sinking.* Once arrived at this point there was no longer any fever; but there was epistaxis, or an exudation of blood from the gums, fauces, or tracheo-bronchial passages, or from the stomach and intestines, in a black and disorganised state.

In the early, or as it may be called the synochal stage, the secretions were in a great measure dried up; but having passed the transition boundary between fever and collapse, though the cutaneous secretion became free, it was cold, and the urine remained in most instances scanty, or was suspended. Thus, it would seem, that the congestion of the first stage in which the cutaneous capillaries were so manifestly involved, found relief in cutaneous relaxation and transpiration, and also in sanguineous exudation from one or other of the mucous surfaces most concerned. During the congestive febrile period, the vital fluid lost its healthy cohesion or plastic properties, and consequently it soon began to ooze through the containing tissues, evidently deprived of their due vital tonicity—no doubt owing to the influence of the same morbid causes which so terribly impaired the vitality of the blood itself.

Here again, as last year, we have to observe the influence of atmospheric changes on the course and tendencies of the same epidemic. As the slight showery weather peculiar to the coast of Peru during the winter months of that region, did not set in this year until the month of July, the duration of the fever was prolonged, and presented some of its worst cases up to the end of June, or the month of July. In July the change of weather to fog and drizzle was established, and then I saw a few cases of what might be called a mixed or hybrid type; preserving the hemorrhagic tendency in the way of epistaxis or exudation from the gums. This kind of complication was seen in fevers attending angina tonsillaris or agues; diseases proper to this change of season. I have seen even the yellow tinge of eye, face, and neck, with dark flocculi in the matters vomited, show themselves with the ordinary symptoms of a well-characterised quotidian in the middle of July.

By way of practical illustration I will now adduce a few cases, which will lay before the reader the transition of the epidemic from its early simple form to another and most aggravated type.

The first case of what might be called yellow fever, into which the prevailing epidemic was now progressively merging, I saw 13th March, in the person of a young lady not long resident in Lima. The disease came on in the manner of a heavy cold, with aching limbs and loins, headache, with suffused eyes and flushed face, on account of which she kept in bed for the first two days without medical advice, and treated her complaint as she would an ordinary cold.

On the evening of the third day I saw her, when she chiefly complained of headache, with the other symptoms enumerated, and the pulse was full, and rather frequent; but there was evidently a disposition to perspire, which, at the time, I took for a most desirable tendency, and encouraged it by warm diluents and diaphoretics. I also ordered a purgative draught for the following morning, because, upon inquiry, I found that her bowels were habitually torpid, and had not been relieved for five days previously. The medicine was taken at an early hour, and when I saw her the next morning it had acted, and the quantity of dark indurated fæces now dislodged was unusually copious, and the relief felt by the patient so satisfactory, that she dressed and went into the drawing-room, thinking herself, in short, quite well, so that no further medical assistance would be required. But in course of the same day she was seized with vomiting of a dark green-looking matter, and betook herself to bed; and having had a cup of warm tea, the vomiting ceased for a few hours, during which she fell asleep. This peculiar vomiting was at the time attributed to the expected appearance of the catamenia, but it turned out afterwards that the reckoning regarding this affair had been erroneous. The vomiting recurred through night, when an effervescing powder was given, but it did not stay the vomiting. Next morning early I visited her, and found that she had one copious alvine discharge during the night, which I saw; it was of a gray colour, and massive consistence. I was also permitted to see what she had vomited at different times in course of the night; it had much the colour and look of coffee-grounds. I had now the windows opened, and curtains drawn, in order to examine the visible signs of disease to the best advantage. I was forcibly impressed by the altered expression of her countenance. Her face and neck were of a deep orange tinge, the eye red and shining, the mind somewhat wandering, but when spoken to, collected and rational in her replies; the pulse was low, and skin cold. A medical consultation of four European practitioners attended, but all to no purpose. In spite of every effort she only survived twenty-four hours more, in course of which time the fatal symptoms thronged upon each other; a convulsive, frequent, lateral motion of the eyes, as well as partial distortions of the face, delirium, laborious breathing, &c., were the precursors of her lamented death.

The case of this young lady was the first of its type that had hitherto occurred in this epidemic, and the members of the consultation were not agreed about its being yellow fever, though I had myself on that occasion called attention to the family likeness between it and the typhus icterodes. Had the nature of the case been suspected at its commencement, perhaps the result might have been different; though it was the lot of all, in the after periods of the epidemic, to watch such cases from their beginning, with an eye to the ultimate result, and yet were not able, in the majority of instances, to render a more favourable report of our success.

On some occasions the patient died in the second stage, without black vomit or yellow skin, and without pain or suffering—in a word, entirely and suddenly *done up;* while, in the first period, there had been no alarming appearances to bespeak the approaching end. The earliest cases of this kind witnessed by me were in young able-bodied Italians lately arrived on the coast. Almost all the fatal cases afterwards observed this season, and terminating in black vomit, took place among individuals of the Indian race recently come down from the cold climates of the Andes, or Spanish Friars, Chileans, and Europeans.

The following case occurred in a robust native in the prime of life, and from the frigid quicksilver district of Huancavilica, beyond the Western Cordillera. He arrived in Lima about the end of May, and fell ill four weeks afterwards. He was a literary gentleman by profession, and was lodged in the house of a wealthy family in the capital, where he received every possible attention. The moment he was taken ill the family physician was sent for, and a few days having passed, the case became more serious; a consultation was therefore called of no fewer than six physicians, who continued their attendance every morning and evening until the eve of his dissolution. I was one of this medical junta; and the family physician related to us at our first sitting, on the fifth day of the patient's illness, that this individual was recently arrived from the lofty Sierra—that the fever entered with cold and rigors, as in cases of ague—that he passed the three first days with high fever, severe headache, and muscular pains of limbs and loins, &c. He was treated with the usual domestic remedies of enemata, pediluvia, and a sudorific regimen, together with such simple medical treatment as the doctor conceived the case only required; and so things went on, until on the fourth day the patient got out of bed, believing himself well, and gave admission to some letters, the perusal of which caused him vexation. His doctor, knowing this, wished to prevent any bilious derangement proceeding from an uneasy mind, and recommended a glassful of iced lemonade. This is the usual drink on such occasions, but it was not kindly received by the stomach, as the patient thought, for shortly after he had drunk it he was attacked by epistaxis, followed by black vomit. Under these circumstances the consultation assembled.

On our examination of the case we found that the patient was in a state of depression of the vital powers, and from the history received we inferred that the *lull* which preceded was only the delusive transition stage of the reigning epidemic in its most dangerous form; and so it proved.

We found him with pulse 60, small, weak, and very compressible, surface cold, skin of a yellow tinge, especially on face and neck, frequent black vomit, with here and there striæ of red blood. All liquids taken (and only liquids were admissible) were soon rejected by the mouth, and apparently without much effort or straining. On pressure there was morbid sensibility observed in the right hypochondrium, a few petechiæ on the arms and body, urine very scanty, bowels slow, and stools of natural formation, but of a darkish tint, gums spongy and inclined to ooze blood, tongue red and tremulous.

Notwithstanding our united efforts in our repeated consultations, the disease continued its own fatal march. The skin assumed a deep yellow colour all over; the tongue became more and more red and tremulous, until it looked exactly like a piece of raw flesh with the blood exuding from it; the eyes red and suffused; gums bleeding; stools darker; urine more and more scanty, and for the last three days of the patient's existence entirely suspended; skin always cold and pulse feeble. On the eighth day, slight sub-delirium was manifested when not disturbed, but spoken to he answered correctly. On the evening of this day the eyes became of a fiery shining hue, with a constant, rapid, lateral convulsive motion, and a remarkable twinkling, which no words can properly express. The whole body began to emit a cadaverous sickening odour, which could only be imperfectly corrected by free aspersion around with the common solution of chloride of lime.

This patient sufferer implored us, latterly, to leave him to die in peace; and declined drink or medicine, as the black vomit never ceased, and was provoked by liquid of any kind. Before the closing scene he felt the hand of death upon him, and made all his arrangements accordingly, and went to his rest on the morning of the ninth day.

Though my main object in relating this case is to represent a strong example in illustration of the inherent tendency to a fatal development thus shown by the epidemic, it will also be proper, for many reasons, to make some remarks in this place on the diagnosis and consequent line of treatment pursued by the junta. One opinion was, that the disease might be as well characterized under the denomination *congestive* fever, as any other general term that could be devised, in our ignorance of the real cause of it. Another thought its phenomena and malignant type might be explained on the supposition of its being a most concentrated form of the bilious remittent, especially as there was, in our first examination, an increase of sensibility, if not also of fulness, observed in the hepatic region. A third, again, emphatically insisted that the black vomit is a symptom peculiar to the yellow fever, and as such, conclusive in the present case, which could be called, as he then conceived, by no other name.

From these opinions it will be seen that up to this period of the second year's epidemic, medical practitioners on the spot were not agreed that, in itself, it was really of the nature of yellow fever, though in so many respects (like the *peste* of the preceding season) similar to that milder form of this disease in its precursory state, so clearly and practically laid down by Dr Copland in his valuable Medical Dictionary, under the title Hemagastric Fever. But occasional cases of malignant nature, such as the one in hand, and a few others of a similar course which had drawn some weeks before the attention of the Prefect, and of others in authority, were, by most of the Lima physicians, yet looked upon as sporadic, and therefore not of sufficient preponderance to justify the epithet *Yellow* to be stamped upon the milder, and, happily, more prevalent form of the reigning fever. As a disease never before experienced in Lima there was an unwillingness (in which I confess I fully partook) to admit its arrival without the most cogent evidence; but now as the cases were becoming too frequent, just as winter was about to set in, and the seasonal epidemic to take its leave, as the year before, the majority of the profession had little doubt about the real character of the enemy at their door; and next year, as we shall see by and by, all scepticism on this subject was removed by the return of the yellow monster early in summer with increased powers of destruction.

Let me now say a few words on the medical treatment of the junta in that formidable case, upon the classification of which there had been so much diversity of opinion; for I look upon this case as initiative of the great changes which were about to present themselves, as well in the form and results of the disease itself, as of medical opinions and treatment in the subsequent history of the epidemic.

The physicians in attendance were educated in different schools, such as Spanish, French, and English. The first formed a majority, and therefore the vote of the junta went in favour of infusion of cinchona, with or without the addition of quinine, vegetable astringents, rhatany and kino, and also opium, with counter-irritants to the gastric region; bits of ice were allowed to dissolve in the mouth, and the aliment thin arrow root. This method of cure was soon changed as ineffectual; and creasote was resorted to in repeated doses of two drops, in order to check the vomiting, but without advantage. The individual who proposed the creasote also suggested the spirits of turpentine, and stated its favourable results in purpura and other hemorrhagic diseases; but as the analogy was not considered satisfactory, and the majority deemed their data not sufficient to recommend its use inwardly, they agreed to apply it outwardly to the abdomen. Alum in solution was used as wash for the mouth. Beer and champaigne wine were also given latterly; but the fatal march of dissolution could not be resisted in spite of these and other diffusive stimuli, such as carbonate of ammonia in dill water, or camphorated infusions of valerian.

So much for the junta case; and let me add a brief account of the general treatment of the epidemic in the current cases of the second year. As to the relative merits of the various modes of cure adopted, it is not easy to form a very exact judgment; for in the milder cases almost all the therapeutics applied yielded equally favourable returns, whereas, in the severer cases, almost all sorts of remedies were equally unsuccessful in their results. In general, as great numbers out of those that fell ill of the epidemic in every family recovered within eight days, every doctor had reason, so far, to be pleased with his own success and treatment. Thus, some lauded their cures effected by the neutral salts, and especially the preparations of potash; while others gave preference to the mineral acids in some aromatic vehicle. If I were to express an opinion of my own, it would be in favour of cleansing the bowels from the beginning with a purgative of rhubarb and calomel in preference to any of a more drastic kind, and afterwards recurring to the salutary native use of enemata as circumstances might demand. The bowels being properly attended to, the next object desired was to promote as free and early a perspiration as possible; for this being attained, the pains, restlessness, and other troublesome symptoms were quickly removed. This end was best secured by the use of aconite and other remedies used in the treatment of the former epidemic, to which I beg to refer the reader. Lemonade, with the juice of the lemon and granadilla combined, formed a beverage always agreeable and refreshing in this as in other fevers. The food was farinaceous at first, and afterwards light chicken soup was advisable; but relapses often arose from imprudence in diet.

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vided line of march—I have not learnt. But I may observe that two of this party returned to the Pacific after having penetrated far into the interior by Truxillo and Cajamarca, and came aboard our steamer, "The Bogota," at Payta. One of these was a Scotchman by birth, and said to be naturally endowed with remarkable fortitude and energy, but so overcome by hardships that he joined us in a state of great exhaustion, and was consigned to a watery grave a few days before we got to Panama. His companion, a strong and ruddy Irishman by birth, we lost sight of at the isthmus. The men of this expedition were among the most powerful, muscular, stalworth samples of human form anywhere to be seen. But when attacked with the Lima epidemic, I observed, that though they themselves made light of the febrile stage, and hardly could be induced to keep in bed on account of it, yet all at once they found their herculean strength abandon them. They would feel listless, and throw their arms carelessly about on the bed, which they no longer wished to leave! The countenance became sunk, the pulse feeble, the skin cold; and when asked, in such circumstances, how they felt, their characteristic reply was, "first-rate;"—but this tranquil hour was but a pause, just before the appearance of black vomit—of longer or shorter duration—convulsions, and death. I have seen, among so many other sick Europeans during this dismal season, a Londoner, sent out to Lima in the service of a gas company intended to illuminate the streets of that remote capital. This was one of the most powerful-looking men I ever beheld, and in the full vigour of life. He landed, with many others of the same company, in perfect health, and died of the epidemic within the week. I saw him for the first time thirty-six hours before death, and a few hours before the black vomit set in, when he insisted that all he wanted was to have a catheter passed—since he was unable to void urine! The fact was that the bladder was empty, and the secretion in question suspended for some time previously, and continued so until his death, which, at first sight, I prognostigated as inevitable.

Others of the gas company were severely attacked—especially a very robust young married woman—after this man's death; but being carefully observed and assisted from the first appearance of indisposition, the course of the fever was modified by medical treatment so much that they all escaped. On the management of the early stage was observed to depend, in a great measure, the avoidance of the fatal tendency in the succeeding period of this epidemic.

I shall now take a nearer view of the subject. The first symptoms of the invasion in this year's fever frequently came on suddenly, and sometimes in the form of cephalalgia, rapidly followed by deliquium, and insensibility continued for several hours. More usually, however, the attack was less precipitate: the patients complained of headache and pain over the orbits, weariness, pains in the limbs and loins, chilliness or rigors; and to the observation of the physician other symptoms presented themselves, such as, eyes red, shining, and expressive of deep apprehension or anxiety; countenance depressed or features contracted, and of a bluish tinge; nausea, retching, or vomiting. The headache, chills, and lumbar pains usually preceded, in order of time, the retching or gastric disorder; and the peculiarly diagnostic pain over, or deeply seated in, the orbits, commonly outlived the headache. When the invasion was soon after meals, it came on attended with vomiting of food, or, in the choleraic form, with cramps, vomiting, and purging.

The initiative state of general perturbation, common to this and so many other fevers, was soon followed by reaction and increased heat of skin, with accelerated circulation, in which the soft and almost empty-feeling pulse, seemed commensurate with the accompanying venous congestion and recoiled perspiration, marked by a dry skin and very notable increase of sensible heat. And so particularly perceptible to the touch of the physician (though little observed by the patient) was this latter symptom, that it came at length to be considered as a prominently diagnostic one. Besides, as I had occasion to remark in the epidemic of the two preceding years, this excess of cutaneous calorification appeared to be quite in disproportion to the relative state of the pulse, which seldom exceeded from 90 to 110, though in a few instances it might even pass 120 in the febrile or first stage. On the third, fourth, or fifth day, this fever, with all its accompanying restlessness, wakefulness, nervous anxiety, and muscular aches, would generally disappear after a more or less free and prolonged perspiration; but then the patient remained languid, though with a quiet, tranquil feeling, which, in too many cases, was a delusive truce, and the forerunner of the hemorrhagic or black vomit state.

In the forms of attack considerable diversity was observable; and though I would rather avoid in this sketch the tedious detail of particular cases, I beg the reader's indulgence while I briefly relate the following one of the much-lamented Dr Galdo, a native of the capital of the Incas

(Cusco), an eminent member of the Peruvian legislature, and in the full prime of life.

This gentleman, when in church, was seized with a fainting fit, and being conveyed home soon after, was affected with cold shivering, followed by fever and severe headache. On the evening of the second day, there was delirium and some stupor. On the morning of the third day, or forty-eight hours after the primary attack, vomiting came with large floculi of a dark colour, interspersed in the liquids rejected by the mouth. On the fourth day, there was still some stupor, and black homogeneous vomiting fully developed itself. On the fifth day, his countenance became disfigured, and eye glassy, but with intellect unclouded, so that he was engaged in reading the Holy Scriptures a few hours before death, which unhappily took place on the evening of this day. His skin was yellow, bowels costive, and only moved by enemata, and urine suspended for the last twenty hours of his existence. His gums were pale and firm even to the last: a very rare circumstance, as bleeding of the gums attended almost every case of this disease, not excepting the mildest forms of it, at some stage or other. The disappearance of stupor as the *black vomit* increased is also remarkable. In numerous instances of the milder sort, I have observed the fever terminate on the evening of the second day, or morning of the third, when the patient would sit up in bed, smoke a cigar, converse cheerfully, and appear to himself and others out of all danger. So much was this the case, that, on many occasions, the physician had hardly left, as he imagined, a convalescent patient, when he was urgently recalled, with the assurance that the same was dying, and the father confessor sent for. At other times, the apparent amendment referred to would be followed with a new febrile accession, but without cold or rigors, which, like the first attack, might be continued over a period of two or three days without remission, and then gradually disappear altogether without any remarkable symptoms, though I have seen such cases terminate now and then in diarrhœa, and after that do well. But I felt disposed, on investigation, to look upon these secondary accessions as so many instances of relapse, proceeding from a too early or too free use of food.

I have also seen the mild form of fever last without intermission till the fifth, or even the seventh day, without any suspicious symptoms, when, both patient and physician being alike deceived, the former would get out of bed, thinking that all his case required was nourishment, and therefore make a good dinner, and rejoice at finding himself out of the doctor's hands. But, alas, how short this hour of complacency! it was speedily followed by hemorrhagic signs, bleeding gums, and then black vomit, rapidly increasing yellowness of skin, general coldness of the body, more or less insensibility of surrounding objects, and, lastly, convulsions gathering in the harvest of death. Thus died, among others, a noble Chilian youth of distinguished family, fortune, and literary acquirements, unavailingly watched over by his devoted countrymen, and assiduously assisted by medical *juntae*, in one of the principal hotels of Lima.

When old people suffered a serious attack, the conflict was soon over after the second stage set in, so rapid was the sinking, even though vomiting did not appear. And another modification I may notice, was that in which, from the commencement of the second stage, this disease took on the enteric form, while the heat of surface always remained low, as did also the pulse. Cases of this description were generally of fatal prognosis; but still I have seen a few recoveries from this class, though with protracted convalescence. We all had but too frequent occasion to witness —and especially in subjects recently arrived from colder climates—a strongly *congestive* form of this fever, the early stage of which seldom excited alarm among the domestic attendants; but to the more experienced eye, its insidious commencement and fatal end were become well known.

This congestive character was generally attended with the ordinary symptoms of the first stage, but with no particularly strong or striking febrile reaction. It soon passed to depression of the vital forces, though I have likewise observed this stage of collapse postponed till the seventh day of the disease; and then would press on a mortiferous array of symptoms: for example, coldness of surface; slow and feeble pulse; a feeling of being really better, or, as the Americans expressed it, "first-rate," but before long followed by a state of indifference or inattention to outward things; coma, more or less profound; a bloated, livid, or swollen and bronzed countenance, upon which swarms of flies enjoyed themselves; lips mottled, blue, and white; a deep and dingy-yellow colour of skin, with livid blotches on those parts most subject to pressure; sometimes the whole body covered with dark petechiæ; bleeding at the nose, or hemorrhagic oozing from the gums; dark stools, solicited by enemata, and, for several hours or days towards the close of the case, scanty or suspended urine; in many instances, picking of the bedclothes, with, finally, convulsions to a greater or less degree, but without *black vomit;* or, if any, only a slight appearance of it in the very agonies of death, which was the inevitable termination of the scene portrayed. Happily this highly congestive variety of the epidemic was recognised so early, or was so much less rapid in its progress, that we were duly warned by intelligible premonitory signs, which guided us to the use of such cordials and stimulants as precluded the transition to that irretrievable condition which I have just depicted. Thus it has been my good fortune to have assisted several patients who recovered from the perilous condition implied by a slight yellow tinge observable about the neck or eyes, spongy and bleeding gums, scanty, high-coloured urine, deep sighing, frequent eructations or hiccup, and general indications of declining vital power.

There remains to be placed on record, as one of the most interesting developments of this year's epidemic, that variety of form in which the patient was seen to outlive the period of primary fever (of a more ardent nature than common), followed by a well-marked remission or pause, about the close of the fifth day, but without apyrexia or total absence of fever. On the sixth day, or thereabout, the tongue became red, raw-looking, and tremulous, with a constant oozing of blood from its surface; from the gums also the blood, oozed as well as from the nose and fauces, or trachea-bronchial passages; and the urine, at first high coloured, became dark but plentiful, presenting a bloody deposit when allowed to rest in a vessel for a short time. I only witnessed two cases of this kind, and both in fine robust young men, of purely Spanish parentage. The one lasted sixteen days, and the other twenty-three days, but both ended fatally.

In each of these instances the fever assumed, and more especially in the latter of twenty-three days, a decidedly typhoid aspect; and the *black vomit,* though profuse and urgent, was entirely and at once stopped in the latter case by repeated draughts of pure fresh and ripe grape-juice; but this remedy had no visible effect on the action of the kidneys, whence decomposed black blood seemed to flow freely in the urine till a week before death. This decomposed-looking blood, which sought the channel of urine as its most convenient emunctory, seemed to have acted as a foreign body in the circulation so long as any of it remained there, and may have, possibly, been an element which contributed to keep up the fever in its typhoid type. The medical junta in long attendance on this particular patient felt persuaded, that, after all hemorrhagic discharges had ceased, the young gentleman might have been restored to his family had he retained the power to digest food or to assimilate new blood. He died apparently from anæmia, or the want of blood enough to carry on the exigencies of his physical nature. His mind was buoyant to the accession of the last convulsive motions, of short duration, in which he expired.

About the end of March, and in the month of April, the epidemic, under the form of black vomit, committed great havoc among troops who had arrived from the bracing climate of Arequipa, a hill-land city in south Peru. These poor fellows bewailed their fate, to have been led off from the assault of Arequipa to die ingloriously of the "vomito-prieto" in Lima. During this period the capital was kept in consternation, not only on account of the revolutionary state of the country, but, more still, by the ravages of the epidemic. The deaths were so frequent and heart-rending, that with the view to give as little publicity to them as possible, the proper authorities prohibited the tolling of bells during the mortal agonies of the dying, who were also to be buried as soon as possible after death, to prevent the accumulation of morbid effluvia.

Through the kindness of Dr Bravo, the zealous physician to the Lima lazaretto, and his devoted assistant, Dr Macedo, I was permitted to witness the *post-mortem* inspection of one of the soldiers from Arequipa, by name Fernando Rivera, on the 1st of April. The following particulars are from my own notes on the occasion :—

Autopsia.—The subject of this anatomical inspection was by blood a "*Cholo*," a cross between Indian and White, of a muscular make, and in the prime of life. He died seventy-two hours from the invasion of the fever, quite delirious, and fighting against the wall to the very last, and his fist remained clenched in death. He was conveyed to the lazaretto after the disease had taken deep possession of him, but *black vomit* only slightly appeared just before he expired. The body for twelve hours before death was as cold as ice, and the dissection commenced five and a-half hours after it.

General appearance.—Skin yellowish, with certain parts —as the ears, nails, and genitals—livid, and the skin, when cut into with the knife, bled easily.

The head.—When opened, the meninges and sinuses of the brain were charged with dark venous blood, but the membranes themselves were quite firm in texture. and free from interstitial deposit of any kind. The ventricles were empty; the substance of the brain was of natural appearance and consistence.

The Thorax.—On opening the cavity of the chest, the lungs were found crepitant under the pressure of the fingers, and, when cut into, saturated with black blood. The heart was of small size; it contained dark, loose blood, and with no appearance of coagula;—indeed, the whole blood in the body appeared to have entirely lost its cohesion, for we saw no coagula anywhere.

The Stomach.—This organ was studded with red papillæ, and looking at these through a good lens, they were distinctly seen more or less sparsely set; and the intervening surface appeared to be clean, even, and of a whitish colour. About the centre of its large curvature, the bloodvessels were in jected more extensively than elsewhere; and it contained in its whole cavity about four ounces of a dark chocolate-coloured liquid.

The Gall-bladder contained about two drachms of a darkish and somewhat oily-looking liquid. Both the common and cystic biliary ducts were all clear.

The *Spleen* was of ordinary size and appearance, and on incision presented nothing unusual.

The *Liver* was externally of a pale straw-colour, but on incision bloodless, and looked exactly like boiled liver.

The *Intestines*, large and small, were to outward view in their natural state; but upon being laid open, the small intestines were seen to contain a dark liquid, such as was found in the stomach; but though brushed over with a wet sponge for the convenience of inspection, they showed no ecchymoses. The large intestines from the cæcum downwards, contained a lighter-coloured or brownish liquid which adhered to their coats; but this washed off easily with water, when they could be examined well, and were found to have a clean surface and firm texture.

The *Bladder of Urine* was empty, and when laid open, the inside looked clear, firm, and without lesion.

The *Kidneys.*—I have no note of their appearance in this autopsia; but my impression is that they presented nothing remarkable in this case, nor indeed in the other numerous observations made in the lazaretto this season. Now it is of great importance to observe that of 19 or 20 cases of autopsia carefully conducted by the physicians of the lazaretto up to the dissection I witnessed, and have just described, the same morbid appearances seen in it were seen in all, with small modifications. For example, in some cases the brain appeared collapsed, or contracted in size; in some other cases the coats of the stomach were observed to be more or less ecchymosed; and the liver, which in our dissection presented a pale external colour, in other instances, however, varied so as to have presented all the hues of the rainbow; and, frequently, it was more or less of a rhubarb-colour.

But after the above dissection, I had notice of three more autopsies which took place some days before I left Lima for England; and in these latter *post-mortem* examinations, besides the general appearances common to all, *one*, in particular, presented a partial softening of the coats of the stomach—but whether or not this peculiarity was the effect of previous disease could not be ascertained; *another*, a female who died of the hemorrhagic form, and was opened an hour or two after death, had four ounces of clear liquid, like water, in her gall-bladder; and in the *third* case, the peculiarity that presented itself on inspection was a liquid like beer in the same viscus.

It was in the more hemorrhagic cases that, on dissection, blotches or ecchymoses were observed in the large intestines. In all the twenty-two or three cases of anatomical observations referred to, a black liquid was found in the stomach and intestines, and the colour of the liver was changed as above expressed.

From the preceding sketch of this year's epidemic, it will be at once seen that neither the symptoms, while the patient was living, nor dissections after death, showed any preponderancy of the bilious type in its nature. The retching and vomiting, rarely presented any appearance of bile; and the absence of bilious vomiting distinguished it from the severe bilious remittents of the hot months, as well as the malignant intermittents of the autumnal changes of temperature on the coast and in the capital of Peru.

Dissections prove that the biliary secretion was mostly, if not altogether suspended, even as, in the latter periods of a fatal case the urine often was so, but in the hemorrhagic form the urine was sometimes free to the last.

The secretion of the liver, like that of the skin, seems to have been dried up from the very commencement of the attack of fever; and the excessive calorification of the skin in the febrile stage may receive some explanation from the fulness of the cutaneous capillaries, which, after death, bled wherever there was an incision made into the skin; and the oppressed breathing, and frequent sighing, may also derive illustration from the congested state of the lungs exhibited by dissection; while, again, the liver when all dry and boiled-like not only did not discover any blood when cut into, but demonstrated its unfitness for the purposes of secretion under the circumstances.

And whether we consider the symptoms in the living, or the morbid appearances in the dead subject, so far as can be reasonably inferred from the dissections made in the Lima lazaretto, the essential morbid changes appeared to have had a deeper seat in the capillaries of the gastro-intestinal mucous membrane than in the brain itself. In some of the worst hemorrhagic forms of the disease, with profuse black vomit, I have observed that the tendency to coma has been very little until the last hours of life, though sub-delirium usually came on some time before death. Coma, or excessive drowsiness, seemed more the attendant of the congestive form, with lips more or less livid, transudation from the gums, and nasal or tracheal mucous surfaces—but without black vomit, though (as evidenced by *post-mortem* inspections) a black liquid might at the same time exist, to some extent, in the stomach and intestines. In the example of autopsy related by me above in detail, the black vomit as mentioned, only slightly appeared just before the unfortunate man died in convulsions.

We have seen that only in a very few instances did the fever become very high, and that though the *pain over the orbits* was an unfailing symptom in the first stage, yet the determination of blood to the head in any transcendent degree, bore, in general, but little proportion to the congested state of the stomach, if we be permitted to judge by the increased irritability of this organ, with its severe retching and vomiting at the first invasion, and then by the repeated profuse discharges, which, with scarcely an effort of the patient, were in the stage of collapse, poured out by the mouth, sometimes in the form of a homogeneous black liquid, or like coffee-grounds, and at other times this same kind of liquid, mixed with the common drink—such as lemonade or cold water—recently swallowed, and speedily ejected with striæ of florid, or apparently undecomposed arterial blood, here and there floating on the surface. Whatever cause produced the fever, appears to have acted very early on the vitality of the blood itself, whether directly or through the medium of the nervous system; for in the few more ardent cases, where I have seen blood drawn in the early period of reaction, it had already suffered evident deterioration, it being of an unusually carbonized dark colour, poor in fibrine, and not able to support its own weight. If the congestive state of the mucous capillaries and consequent transudation of disorganized blood be attributed to local inflammatory action—or as some would term it, gastro-enteritis—what was it that from the very beginning of the febrile stage diminished the cohesion between the sanguineous molecules themselves, so as to have affected the whole circulating mass, as is borne out by phlebotomy? Can the local inflammatory action, *per se*, be supposed to produce so great a change on the capillaries and the blood congested in them, and yet so little on the mucous tissues as we learn from dissection? If the morbid changes in the mass of blood, as well as that contained in the capillaries, cannot be explained by any hypothesis relating to the forms of inflammatory action, it must be assigned to something peculiar—and as yet inexplicable—in the causé, or in the nature of the fever itself; which, in point of arterial action, falls much short of the highly augmented movements of the heart and general circulation so usually manifested in common remittent or intermittent fevers; for these—as witnessed in Peru at least—never, except in their most isolated and malignant form, have been seen at any period on record, attended with the fatal symptom of black vomit. And I may add, that I myself, during a long period of full practice in that country, never assisted any such sporadic bilious remittents of malignant nature, without having had room to observe unequivocal marks of local inflammation, such as in the present epidemic have not been satisfactorily discovered, so far as I know, either in the living or the dead.

I must not omit to state, that this epidemic, which lasted through three successive years, and steadily progressing towards its full development in the manner now before the reader, had one striking point of difference between it and the ordinary accounts of yellow fever we hear and read of, and that is, that one attack did not appear to be a sufficient protection against a second, if not during the same, as was sometimes reported to have occurred at least in the following seasons. It is probable that the double attacks were only relapses rather than repetitions in the same persons, during the same year; this proposition I could not solve at the time to my own satisfaction; such cases were rare. I myself suffered an unusually severe attack of the first year's epidemic, which terminated in an exhausting cold sweat and scarlet eruption, with slight sanguineous exudation from the tracheal mucous membrane, and followed by weeks of lassitude and imperfect convalescence; but this did not protect me from another and far more severe attack in the month of May of the following year, just as the epidemic was merging into an acknowledged yellow fever, or *vomito prieto*, by the marked cases which here and there assumed this character. It did not arrive at this point with me; but the transition from the state of fever to that of extreme collapse (in which I vomited a mouthful of glary matter, like the albumen of an egg, and which tasted of concentrated mineral acid) nearly cost me my life; and as reaction returned, hemorrhage at the mouth and on the lip initiated itself, and there happily ended; but the third year, when it raged so fatally, I entirely escaped. My family and domestics had it the first year, escaped the second, and had it again on the third year. One of my servants, a Negro, had it very mildly; another, an Indian, much more severely; and a third, a Chinese, also had it moderately; while my wife, who had been dangerously ill of the first year's epidemic, escaped the second, and now, the third, was so ill as to have found it requisite to return to Europe in order to recruit her strength. But this is bringing the matter home to one's own door; while as a physician working among those laid up in every street in Lima, I had constant occasion to remark that the same individuals had suffered in a greater or less degree more than once, and that the epidemic of the first year did not protect from the visitations of the second or third year. It remains now to be revealed by time whether those who had recovered from the severer forms of this scourge in 1854 will be safe from its future ravages, or whether last year, in its fury, the disease may not have exhausted itself. By recent letters from the seaport of Lima, I am able to state that, up to the 11th of January at least, notwithstanding the great heat of the season, and the triumphant entry of General Castilla's army, no yellow fever had appeared. Last year it recurred earlier, and, therefore, my correspondent says they all hoped to be spared from its ravages during the summer and autumn of 1855.

With these remarks I shall pass on to offer a few observations on the treatment adopted in this epidemic of most engrossing interest in the year 1854. I believe that all the physicians, native as well as foreign, who assisted in this fever—of whom several fell victims in their discharge of arduous duty—were agreed upon the necessity of having the bowels cleared out in the first place, and with the least possible delay. For this purpose some preferred one kind of purgative, some another. One would laud above all things castor-oil, or almond and olive oils, with lemon juice; another would give saline purgatives, to which some were fond of adding tartar emetic, in order to produce not only vomiting and purging, but a powerful perturbation of the whole frame. But seeing that natural vomiting attended the invasion, or, at least, that no drug was needed to produce it, native practitioners, especially, thought it better simply to favour the efforts of nature by drinking freely of tepid water in the first instance, and after that administered twenty or thirty grains of rhubarb, with three or four grains of calomel as a dose, to be followed up by enemata from time to time, as the subsequent course of the disease seemed to demand. After the bowels were thus acted upon, little more was required, *in a large proportion of the ordinary kind of cases*, than the practice of the preceding year—viz., the sudorific regimen, with the use of aconite, antimonials, and hyoscyamus, &c., to allay pain, to soothe the nervous system, and promote perspiration, and a happy recovery. But in too many other cases such treatment was not enough.

Bleeding.—General depletion by the lancet was tried with confidence, probably founded on European opinions, by a celebrated Spanish physician just arrived from the peninsula; but scarcely was the arm bound up in these cases when the strong and full pulse of the preceding hour began to sink with extraordinary rapidity, especially if the bleeding took place after the second day of the fever. By this time, however, the other European physicians of longer experience of the climate, and particularly with the experience of the two preceding seasons of the prevailing epidemic, had learned more caution on this subject; for it was a settled opinion among them, as with the natives, that bleeding could not be resorted to without incurring thereby the utmost danger to the patient. Even local bleeding was injurious, and on account of the hemorrhagic tendency of the disease, it was hardly possible at times to stop the bleeding from the leech bites. Bleeding was therefore proscribed as a practical rule; and Dr Paredes, as I have already stated, informs us that it was also disapproved by our professional predecessors in the somewhat analogous Lima epidemic of 1818.

Cold Bath.—In those who survived the black vomit, and with the returning tide of life had to undergo a lively reaction, cold immersion was practised according to the powers of the individual, and when judiciously regulated, apparently with good effect. Tepid aspersion or ablution with the sponge in hand, in cases of moderate reaction, was comforting, if not decidedly beneficial; but few native patients could have borne the full shock of the cold affusion, and, in general, even cold ablution was not admitted in the ordinary plan of cure.

Wet Sheet.—At the English hospital of Bellavista, near Callao, appointed for the reception of British and American seamen and subjects, strong able-bodied men, but with constitutions unacclimatised, were daily admitted from the seaport. The practice at first adopted in this establishment, as I state on the authority of Dr Gallagher, its experienced physician, was large and repeated doses of calomel, under an impression that the disease arose from a congested or deranged state of the liver, but every case treated in this way was lost. He and his assistant therefore abandoned the calomel treatment, and had recourse to the water system with better success. Dr Gallagher assured me that when the wet sheet was resorted to early, and continued during the hot febrile stage in these strong European inmates of his hospital, the fatal tendency of the transition to the second stage was disarmed of danger in a very great proportion of cases; and the injection of cold water into the rectum was also attended with better effects than they had previously derived from drastic purges.

Cinchona Bark.—Towards the close of the second year's and in the early weeks of the third year's epidemic, infusion of this bark, with the addition of sulphuric acid, or full doses of sulphate of quinine, were applied with great diligence, as well by the mouth as in the form of enemata, by practitioners of the Spanish school. This treatment, pushed to its utmost in hospital practice—on the idea, I believe, that the fever might be essentially of the nature of the West Indian remittents, though its actual form was continued—was abandoned even by Dr Rios himself, the very talented physician of St Andres, as affording no ground of confidence.

When the epidemic of 1854 had commenced in Callao, and not many days before it appeared in Lima in the month of January of that year, I felt it to be my duty, as well as pleasure, to present the head physician of the lazaretto with a copy I fortunately had by me of Dr Copland's Dictionary of Practical Medicine, in order that this eminent writer's curative plan in the hemagastric or pestilential yellow fever—a disease which he appears to have studied with the greatest practical care—should have a fair trial in circumstances so very like those depicted in his masterly treatise on this subject.

This incident now led to the general use of the spirits of turpentine in the hemorrhagic form of the epidemic, and, when practicable, was used towards the close of the first stage, before the vital powers showed signs of sinking, and therefore before hemorrhage or black vomit had actually commenced. Unfortunately patients sent to the lazaretto did not arrive until the second stage was fully established in all the details of its deplorable character. But notwithstanding such adverse conditions, the result of the turpentine, administered in doses of ʒj. by the mouth, or ℥ss. by the anus (for in this advanced stage small doses were of little efficacy), several times daily, saved—according to the undoubted evidence of the gentlemen in charge of this charitable establishment—not fewer than 50 per cent. of the Indian race admitted in the last stage with black vomit, while the same remedy had but comparatively little power of curing this disease in the white or European race. In the negro there was no opportunity of trying its potency, as in this race the fever was almost always ephemeral. The other coloured and mixed breeds of different shades between the African, Indian, and White were not exempt from the hemorrhagic form with *black vomit*; but in them, too, the turpentine cure was often successful, though not so much so as in the pure Indian. This well-authenticated fact is important in itself, and may afford scope for speculation among those who take interest in tracing the influence of the distinctive constitutional varieties observed in the human family on the history and development of disease.

In the decline of the febrile stage the infusion of valerian with ether, camphor, or musk, in combination, was very generally used, with a view to support the vital powers from the approaching period of sinking or depression. Carbonate of ammonia, or ammoniated tincture of valerian, in any convenient aromatic vehicle, were employed for the same purpose; and not only in the hospitals, but in private practice likewise, pills, consisting of opium with creosote, or camphor, capsicum and opium, &c., as prescribed by Dr Copland, were administered, with close attention to the result. In a vast number of cases this practice was attended with the best effects; for it kept up the vigour of the circulation, at least to the degree of not allowing the surface or extremities to become cold, and prevented the realisation of the often-threatened gastro-enteric exudation of black vomit or disorganised blood. In more advanced cases, though not resorted to even until the gums exuded blood, and yellow skin, with hiccup, and failing pulse, were all present, it often arrested the fatal march of the disease. Nay more, I have seen the black vomit announce its approach by dark-gray flocculi in the liquids ejected by the mouth, and yet further progress suspended by pills of one half grain of opium and two drops of creosote, administered every five or six hours, till all bad symptoms ceased, and a healthy, moderate reaction supervened. When the pills created pyrosis, or uneasy heat at stomach, they were dropt. When thus the peril of the disease was removed or obviated, I have found the greatest benefit from the taraxicum as a restorer of healthy intestinal action and due biliary secretion. For an indefinite period, notwithstanding the recovery might be favourable, the gums were spongy or bleeding at times; and the yellowness of the skin seemed to be the last symptom to take its leave.

In the *White race* I have constantly noticed that turpentine, in moderate doses of from ʒss. to ʒi. three or four times in course of the day, stopt the hemorrhage from the mouth and stomach, but not from the urinary passages or intestines, not even when administered in the latter case by enema; probably because it could not be made to reach the small intestines so as to act thereon directly, or produce its local styptic effects.

Externally, turpentine was also in frequent use as a counterirritant in preference to blisters of cantharides; for in a low state of the vital powers, the latter might be followed by gangrene.

Enemata of all sorts were used according to circumstances, just as the indication was either to clear the bowels, restrain the hemorrhagic exudations, or support the vital forces and nourish the system; thus cold water, iced, was applied in the hemorrhagic forms as well by the mouth as the anus, as also infusion or decoction of cinchona bark with the addition of valerian, camphor, or opium, &c. Beer, wine, brandy and water, were called to assist as cordials, while rice-water, arrow-root, sago, or light chicken soup, were employed as nourishment. But the solemn truth must be avowed that, the instances of recovery from a fully established case of black vomit in the purely *white race*, whether native or foreign, and under whatever mode of treatment, were very, very few. For months together I must have visited, one day with another, at least one hundred patients daily during the epidemic, when in almost every family more than one sick was laid up at the same time, and in many houses it fell so heavily, that almost all would be laid up together; and yet among such a profusion of patients (embracing a great many assisted in consultations with other physicians), I can only recall to mind three decided instances of *a white man* having reached the stage of full, homogeneous, black vomit, and yet to have survived that danger. In the mixed races of various shades of colour, I have witnessed greater success; but it was in the pure Indian, when the transition from reaction to prostration of strength, with refrigeration of skin and all other symptoms of collapse were present, and the hemorrhagic form predominant, without any lingering remains of a feverish pulse or heat—it was in this race, as before mentioned, and in these circumstances, that the turpentine cure proved to be so pre-eminently successful.

In those cases, on the other hand, which I have noticed as of a typhoid type, or where the pulse maintained itself frequent and feverish throughout the whole course of the case, little hope, even in the indigenous Indian race, was to be entertained from the turpentine; while in the more favourable cases for its administration, it seemed to have acted *locally* as a powerful styptic, and *generally* as a diffusive stimulant to the sinking vital powers.

In a disease which so lamentably baffled the best efforts of the profession on so many fatal occasions, it will be well to bear in mind the various remedies used in order to guide others in the way of improvement when their turn comes. I shall therefore mention that in one case of a marine officer which had just occurred a few days before I left Lima, turpentine, creosote, &c., had been used in vain to stop the black vomit, when it occurred to one of the physicians in attendance to suggest the trial in so desperate a case of the *sulphite* of soda, in small repeated doses; the result was stated to be perfectly successful, and this young gentleman of Spanish blood was numbered among the saved. But, however interesting such a fact may prove in itself, no general conclusions can be laid down from a solitary case; for by the use of only rice-water, with the addition of fresh lemon juice as food and drink for several days, and enemata of mallow water with the juice of the common garden herb *verbena*, I have known a cure of the black vomit effected in one of the orchards of the suburbs, remote from medical aid; yet, in numberless other instances of the same epidemic, these popular remedies were tried in vain. The same observations apply to the fresh juice of the grape.

While this epidemic reigned in Lima, people from the inland valleys of the Andes and from both sides of the Cordil-

lera, had daily commercial intercourse with the capital; and many of them were seized by the fever on their way home to the interior. Of these, not a few died as they reached the foot of the Cordillera; and though I heard of one who crossed the mountain pass and got as far as *Jauja*, about fifty leagues from the coast, and died of the fever after all, yet I have learnt of no instance where it was conveyed from one to another on the cold table-lands. The *vomito prieto*, or even the epidemic of which it was so marked a symptom, was, if I be allowed the metaphor, a chick that could not break its shell without a more fostering warmth than that of the lofty Sierra.

In conclusion, I shall briefly notice that at this very time a letter was received in Lima from the Prefect of the distant province of Conchucos which excited deep interest, as it was feared that the same epidemic which filled the capital with mourning had also broken out among the Andes. It appeared that at Llumpa and Masqui, two villages in Conchucos, and situated in warm glens whose waters bend their course eastward to the great basins of the Amazons, there had prevailed since the month of January (1844), an epidemic disease which carried numberless victims in its train; and rapidly spread from north to south until it reached the village of Chacas, situated in a cold and elevated neighbouring district. But the Indians of these parts took the alarm, and put all the affected places under strict quarantine, to prevent the further diffusion of the dreaded malady, the origin of which was assigned to unusual rains; so that the ground becoming swampy, and again acted upon powerfully by solar heat, was believed to have engendered a deadly miasmatic poison.

The symptoms of this inland epidemic, so far as could be collected from non-professional reports, consisted of pain in the bowels, with vomiting and purging—probably cholera.

Medical Notes on the Climate of Burmah, and on the Diseases which have there prevailed among European Troops. By CHARLES MURCHISON, M.D., Physician to the Westminster General Dispensary, and late of the Bengal Medical Service.

(Continued from p. 85.)

In my former paper I made some observations on the geographical and geological position, and on the climate of Prome, mentioned a few particulars connected with the arrival of the 2d Bengal European Regiment at that station in 1853, enumerated the diseases which prevailed in the regiment for some months after its arrival, and brought forward a few of the principal causes to which these diseases might with propriety be attributed. I now, in the fifth place, proceed to give a few more detailed observations on some of these diseases.

The great preponderance of febrile over other diseases has already been pointed out, upwards of 75 per cent. of the admissions into the hospital at headquarters, for the first three months after the arrival of the regiment at Prome, and upwards of 79 per cent. in the detachment sent into the district, being on account of this class of diseases. These fevers may be said to have been of two forms, the intermittent and remittent; for, though a few cases of continued fever were noted, yet, as already mentioned, they were very few in number, and closely approximated in characters to the remittent fevers, in all but the absence of a well marked period of remission. The number of cases of intermittent fever during the three months was 219; of remittent, 105; and of continued, 4; while, of the eighty-six cases of fever occurring in the detachment, just alluded to, seventy-nine were intermittent, and seven, remittent.

I shall, in the first place, make a few observations on the intermittent fevers, and afterwards on the remittent, contrasting the characters of the latter with those of the continued fever, which prevailed in the same regiment at Calcutta during the hot, dry months of May and June of the same year.

I. *Intermittent Fevers.*—Of the 219 cases of intermittent fever admitted into the hospital at Prome, 178, or 81·3 per cent. were quotidian, and 41, or 18·7 per cent. tertian; and of the 79 cases in the detachment 71, or 89 per cent., were quotidian, and the remainder, tertian. It must be observed, however, that a few of the quotidian cases might be called double quotidian, and a few of the tertian cases presented the form of what is called the double tertian, but care was taken to distinguish between these latter and quotidians. Again, a very few of the cases noted as tertian passed into the quotidian form, while a considerable number, about 5 per cent., of the quotidian cases, under the influence of treatment, became tertian, before a complete cure was effected; and again, two or three cases of decided quotidian intermittents at first were observed to pass into the remittent form. The above proportion of tertian to quotidian cases is very different from what we find laid down in books on the subject. Thus, Dr Watson, in his lectures,[1] observes, "the tertian is by much the most common." Dr Copland says, "The most common of these is the tertian, which is considered the primary type of fever."[2] In the article, "Intermittent Fever," in Tweedie's Library of Medicine, the author states, "The quotidian but rarely occurs, and that a too hasty observation has confounded it with the double tertian form."[3] Again, we find the following sentence in notes which we took from a clinical lecture by Dr Christison, March 19, 1850:—"Tertian is the most frequent, and hence may be regarded as the type." Among the intermittents which are met with in this country, the tertian is no doubt the most common form, but we believe this is seldom the case in the intermittents of India. As corroborating the results obtained by myself, I may mention those which have been lately published by Mr Waring of the Madras Medical Service.[4] Out of 240 cases noted by this gentleman,

174 or 72·5	per cent. were	quotidian,
3 or 1·25	,,	double quotidian,
47 or 19·58	,,	tertian,
6 or 2·5	,,	quartan,
10 or 4·16	,,	irregular.

It would be quite unnecessary to give any detailed history of these cases of intermittent fever, which only differed from ordinary cases of intermittent in the unusual tendency which many of the cases exhibited to a state of great prostration. Many of the cases, indeed, exactly coincided with the characters of Dr Copland's third species of intermittent fever, which he designates, "ague, with oppressed power."[5] Thus, in the cold stage there were often no rigors, but only a feeling of chilliness, with or without slight horripilations. This feeling was accompanied or followed by great nausea, tenderness at the epigastrium, sometimes vomiting, a quick, small pulse, headache, pains in the back, moderate thirst, and dry, sallow skin. During the intermissions, too, there was great debility, a loaded tongue, sallow skin, morbid excretions, and headache. The following observations, however, may not be without interest, as bearing upon the hour of the commencement of the paroxysm, the frequency of certain symptoms and complications, &c., and the best mode of administering the quinine.

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[1] Vol. i., p. 723.
[2] Dict. of Pract. Medicine, vol. i., p. 935.
[3] Vol. i., p. 219.
[4] Indian Annals of Medical Science, October 1853.
[5] Dictionary of Practical Medicine, vol. i., p. 936.

In the first place, I shall speak in reference to the *quotidian* intermittents. Of 115 cases of quotidian intermittents which came more immediately under my own care, I took careful notes, and from these notes have constructed a table,[1] of which these are the results, as bearing upon the following points:—

1. *The hour of commencement of the paroxysm.*

In 1	case the paroxysm commenced at	4 A.M.
1	... ...	5 ...
2	... ...	6 ...
4	... ...	6½ ...
4	... ...	7 ...
9	... ...	8 ...
3	... ...	8½ ...
15	... ...	...
23	... ...	1 ...
19	... ...	1 ...
5	... ...	noon.
8	... ...	1 P.M.
9	... ...	2 ...
6	... ...	3 ...
1	... ...	4 ...
1	... ...	5 ...
1	... ...	6 ...
1	... ...	midnight.
2	hour of commencement not determined.	

The hours noted, are those at which the paroxysms commenced before admission into hospital, or before treatment. After the commencement of treatment, the paroxysms, if not at once checked, became gradually later in their hour of accession. It will thus be seen, that in 86 out of 113 cases, or in 76 per cent., the paroxysms commenced between midnight and noon (inclusive of the latter), and in 27 cases, or 24 per cent., between noon and midnight. The most frequent hours of commencement were 9, 10, and 11 A.M.; in 74 cases, or 65·5 per cent., the paroxysms commenced between 8 A.M. and noon inclusive. After these, the most frequent hours were 1, 2, or 3 P.M., 23 cases, or 20·3 per cent., occurring at one or other of these hours. These were undoubted cases of quotidian, notwithstanding Dr Shapter's doubts if the paroxysms of true quotidian ever commence after noon.[1] Of the remaining 16 cases, in 12 the hour of commencement was before 8 A.M., and in 4, after 3 P.M. It will be observed that in one of the cases, the paroxysms commenced at the very unusual hour of midnight. This man attributed the attack to a cold draught which blew upon him through the door of the quarter-guard, in which he was a prisoner, but it is worthy of remark, that in the case of the same patient, when he had ague nine years before, the paroxysms also came on at midnight, without any such cause being in operation.

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[1] Owing to its great length, the table has not been published.

We find that the above results correspond with the statements laid down by most writers on intermittent fever. Thus Dr Copland remarks,[2] "The quotidian usually begins in the morning." Dr Watson observes,[3] "The paroxysms of the quotidian ague begin in the morning;" while Dr Shapter, in Tweedie's Library of Medicine,[4] goes so far as to say, that it is doubtful if the disease can ever be considered a true quotidian, if it commences after noon, or during the night; but we have just seen, that in upwards of 20 per cent. of our cases, the paroxysms commenced between 1 and 3 P.M. inclusive, and we may mention that Mr Waring, of the Madras Medical Service, has recently recorded, from observations made on the intermittent fevers prevailing among the Burmese at Mergui, results very different from any of the above. He found the most frequent hour for the commencement of the paroxysm to be 2 P.M., and out of 240 cases the paroxysm commenced—

Between noon and midnight,	in 156 cases,	or 65 per cent.
Between midnight and noon, (including latter hour),	in 67 cases,	or 28 per cent.
Irregularly,	in 17 cases,	or 7 per cent.

Mr Waring's results refer to all the species of intermittents; but, we have already seen, that 72½ per cent. of his cases were quotidian.

2. *Presence and severity of rigors.* In order to form some idea on this matter, I have adopted a somewhat arbitrary classification of the cases, under the heads of severe, moderate, slight, or absent. We find then, that—

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[1] Tweedie's Library of Medicine, vol. i., p. 219.
[2] Dictionary of Practical Medicine, vol. i., p. 935.
[3] Lectures, vol. i., p.723.
[4] Vol. i., p. 219.

In 28 cases, or 24·3 per cent., the paroxysms commenced with *severe* rigors, lasting generally from one to two hours.

In 50 cases, or 43·4 per cent., the rigors were *moderate*, lasting from one half to one hour.

In 13 cases, or 11·3 per cent., the rigors were *slight*, or lasted only from five minutes to half an hour.

In 24 cases, or 20·7 per cent., there were no rigors.

We thus see that in upwards of 32 per cent. of the cases, the rigors were either absent or very slight, and we have already stated, that this has been observed to be characteristic of a low, adynamic form of intermittent. In these cases in which the rigors were absent, the hot stage was preceded only by a feeling of chilliness over the surface of the body, sometimes accompanied by creeping sensations, at other times by a feeling of numbness. Sometimes it was general over the surface of the body, at other times confined to the extremities, sometimes it lasted for several hours, but generally only for one or half an hour. It may be mentioned also, that those cases, in which the rigors were absent, were generally men with the most debilitated constitutions. In 24 per cent. the rigors are noted as severe, but, as a general rule, the intensity of the cold stage bore no relation to the severity of the rest of the paroxysm. In four of the cases, though the fever returned every day at the same hour, it was only preceded by rigors every second day, the fever in these days coming on either at once, or being preceded only by slight chilliness. These have been classed with the cases in which the rigors were moderate, though, perhaps, with more propriety they should have been designated double tertian fevers, yet the other characters of the fever were precisely similar on each day. Of 240 cases of intermittent, treated in Burmah by Mr Waring, in 124 there was a distinct cold stage, while in 116 there was not.[1]

3. *Headache* was an almost constant accompaniment of the hot stage. In one case only was it noted as entirely absent. This was the case before alluded to as peculiar in consequence of the fits of ague coming on at midnight, and in this case, there was scarcely any hot stage, perspiration succeeding almost immediately to the cold stage. Of the other 114 cases, in 9 the headache was noted as very violent; in 77, severe; and in 28, slight. The headache generally subsided or disappeared entirely during the third stage. In many cases, however, it remained rather severe during

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[1] Indian Annals of Medical Science, October 1853.

the whole of the intermissions, and in two of these cases it was ultimately relieved by copious epistaxis.

4. The fourth column in the table is intended to show the frequency of *enlargement of the liver and spleen.* It may be mentioned, that in almost every case in which the spleen was carefully examined by means of percussion, it was found to be considerably enlarged during the paroxysms, the enlargement subsiding spontaneously during the intermissions, whether quinine was administered or not. Those cases only, however, have been particularly noticed in which the splenic enlargement was considerable, permanent, and attended by pain or great tenderness, forming the subject of complaint by the patient, and sometimes requiring for its relief local depletion or counter-irritations. This was the case in 22 out of the 115 cases. The liver, as well as the spleen, was almost invariably slightly enlarged, and tender on pressure during the hot stage, owing, no doubt, to congestion of the portal system; but in nine cases there were symptoms of inflammatory action, the enlargement being considerable, permanent, and attended by pain and great tenderness, requiring for their relief local depletion and counter-irritation. Three of these nine cases corresponded with those in which there were also symptoms of inflammatory action in the spleen. The enlargement of these abdominal organs was, no doubt, in many cases the cause of the vomiting during the paroxysms, from their pressure on the stomach, while in others they seemed to give rise to a spasmodic cough, from their pressure on the diaphragm.

5. The fifth column indicates the frequency of such abdominal symptoms, as *nausea, vomiting, diarrhœa, &c.* All the cases were attended by more or less nausea during the paroxysms, but in 35 out of the 115 patients, or in 30.4 per cent., this symptom was particularly severe. In 28 out of these 35 cases, the nausea was accompanied by vomiting, the vomited matter being sometimes almost pure bile; 11 of these 28 cases corresponded with those already mentioned, as exhibiting symptoms of inflammatory action, either in the liver or spleen. The vomiting generally came on towards the end of the first stage, lasted during the greater part of the second, and subsided in the third. Sometimes, however, there was vomiting throughout the first or cold stage, and in one or two cases the paroxysms were ushered in by vomiting before even the rigors commenced. In two cases the vomiting continued during the intermissions, being apparently aggravated by the substitution of the pulvis cinchonæ for quinine. In 3 cases there was great flatulence, with severe abdominal pains. In 5 cases there was diarrhœa; in 4 of

these 5 cases there were also symptoms of hepatitis ; in the fifth, there were none.

6. In the sixth column I have noted the occurrence of *pulmonary complications*, which were by no means frequent. In 2 cases there was slight bronchitis, characterized by a wheezing cough, slight mucous expectoration, with sibilant, and a few moist rales. In 2 cases there was a severe spasmodic cough, with a feeling of great oppression about the chest, which symptoms invariably appeared at the commencement of each paroxysm, and subsided towards its termination. In one case there was pharyngitis, which came on the third day, and was easily subdued.

7. The *urine* in four or five cases in which it was carefully examined, was invariably found to be scanty, dark-coloured, acid, of high specific gravity, and containing no albumen, during the paroxysms. No unusual deposit of lithates was found during the intermissions, or in convalescence.

8. In the eighth column is shown the occurrence of *nervous complications*. Pains in the back and loins were by no means uncommon, but in 5 cases they were peculiarly severe, and formed the subject of great complaint by the patient. In one case the paroxysms were invariably accompanied by severe pains in all the joints. In another there were severe deep-seated pains in the bones. In one patient, during two separate attacks, the hot stage was always accompanied by severe cramps in the feet and calves of the legs, these parts remaining very tender for some days after the paroxysms had been checked. In another case, the paroxysms were always attended by violent neuralgic pain confined to one spot situated about an inch behind the lobe of the right ear. This man stated, that the same distressing symptom had accompanied previous attacks of intermittent fever in Bengal. It was generally much relieved by an opium plaster.

9. *Cutaneous Eruptions.*—In one case, during the second paroxysm, an eruption of urticaria came out over the arms and thorax, disappearing next day, and followed by a herpetic eruption round the lips, which continued for some days. This same patient had a complication of organic diseases, hypertrophy of the heart, liver, &c., and it was observed, that, during the hot stage, the pain in the cardiac region was always very much increased.

10. *Epistaxis.*—In 3 cases, two of which were double quotidians, there was copious epistaxis. In all these cases there had been violent headache, to which the epistaxis gave great and immediate relief.

11. As regards the *premonitory symptoms*, those most frequently observed, were yawning and extreme lassitude for from half-an-hour to several hours before the commencement of the cold stage. In a few cases the patients felt dull and listless, for several days before the first paroxysm. In some cases these rigors were preceded by great chilliness, and creeping sensations for some hours, while in not a few cases, there were no premonitory symptoms whatever. In several cases, the paroxysms came on during a convalescence from remittent fever, or previous attacks of intermittent.

12. The next column shows the *duration of the paroxysms* in the cases in which this was accurately determined. This was done in 95 out of 115 cases, with the following results—

In 1	case the paroxysm	lasted 2	hours.
2	cases ...	... 3	...
2	... ...	... 4	...
14	... ...	... 5	...
14	... ...	... 6	...
20	... ...	... 7	...
27	... ...	... 8	...
4	... ...	... 9	...
6	... ...	... 10	...
1	... ...	... 12	...
3	... ...	... 14	...
1	... ...	... 16	...

In estimating the duration of the paroxysms, the third stage was scarcely included, but the duration was counted from the commencement of the rigors until the full establishment of perspiration. We thus see that the ordinary duration was 7 or 8 hours; the longest, 16 hours; the shortest, 2; and the average, $7\frac{87}{95}$ hours. This is somewhat under the duration laid down by most writers. With regard to the duration of the paroxysm, Dr Copland, speaking of intermittents generally, remarks, it "varies from 4 to 16 hours, the common length of it being about 6 or 7," while that of the quotidian "continues the longest."[1] Dr Watson says, "The average duration of the paroxysm the quotidian is 10 or 12 hours;"[2] and Dr Jos. Brown says, "It will be very near the truth, to estimate the mean length of the whole paroxysm at 16 hours."[3] But this estimate would appear to be rather above the mark.

13. The next column shews the number of days each patient remained in hospital after the last paroxysm; and it was drawn up, in order to form some idea of the degree of

[1] Dict. of Pract. Med., vol. i., 935.

[2] Lectures, vol. i., p. 723.

[3] Cyclopædia of Pract. Med., ii., p. 221.

debility produced in the different cases, by considering the length of time, which elapsed before the patient was able to return to his duty. After his discharge from hospital, each patient was in general excused from duty for two days.

3	Patients were	discharged on the	1st day	after last paroxysm.
17	... ...	...	2d	... ...
21	... ...	...	3d	... ...
44	... ...	...	4th	... ...
10	... ...	...	5th	... ...
12	... ...	...	6th	... ...
2	... ...	...	7th	... ...
1	... ...	...	9th	... ...
1	... ...	...	10th	... ...
1	... ...	...	12th	... ...
3	... ...	...	15th	... ...

Most of the patients were discharged before the fifth day, and all, except eight, before the seventh. The average stay was 4^{29} days. Those detained in hospital longer than the usual period, had some organic disease in addition to the fever.

The remaining portion of the table has reference to a very important point in the treatment of intermittent fever, viz., the best way of administering quinine, so as to prevent a return of the paroxysms. Some have recommended the administration of the quinine during the paroxysms, but this is a practice which few have ever followed. It has been a matter of considerable question, however, whether the quinine should be given in one large dose, or in repeated small doses during the intermissions. Dr Home of Edinburgh, from his experiments, pronounced in favour of the repeated small doses, as also Dr Barker of Dublin.[1] Dr Brown, in the Cyclopædia of Practical Medicine,[2] recommends two grains every three hours, or four every six, during the intermissions; but he allows, that in those cases in which "life probably depends on the prevention of a paroxysm," it should be given in much larger doses, such as a scruple. Dr Watson[3] of London is also favourable to the repeated small doses, amounting to 12 grains in the 24 hours, which plan, he says, he has found most successful, though, at the same time, he acknowledges, that a "very few paroxysms have occurred after the patient has begun to take the medicine." He also urges this plan on economical grounds, it being our object, he says, to make the cure "as cheap as possible." On the other hand, we have practitioners recommending large doses. Dr Elliotson gave a large dose just after the paroxysm, and smaller repeated doses during the remainder of the intermission, amounting, in all, to 20 or 30 grains in 24 hours.[1] Dr Copland recommends a full dose, 6 to 8 grains, immediately after the fit, or shortly before its return, or a large dose followed by smaller doses every three or four hours.[2] Dr Shapter, in Tweedie's Library of Medicine,[3] remarks, "Some physicians have administered it to the extent of 20 grains at a dose, and have by this means succeeded in putting an immediate stop to the disease."

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[1] Dr Watson's Lectures, vol. i., p. 765.

[2] Vol. ii., p. 220.

[3] Lectures, *loc. cit.*

Dr Christison, in his Dispensatory,[4] says, the intermittents of the Tropics require 36 or 40 grains on an average; and, in a clinical lecture delivered in Edinburgh (March 19, 1850), he stated, "It is better to give a large dose at once, such as 36 grains, which has been shown to be the average amount required in India." Dr Christison informs me, that his calculation, that 36 grains was the average dose necessary for the cure of tropical intermittents, was deduced from reports on the febrifuge virtues of quina, made by medical officers of the Madras army, at the request of the Medical Board, and published in the Madras Medical Reports for 1831.

In America, the medium dose is stated by Dr Watson to be eight grains.[5]

Continental practitioners seem to be also in favour of the repeated small doses. Thus Rayer, in his article on fever, speaking of French practitioners, says, that though sometimes 5, 10, 20, or even 30 grains are given at one dose, yet the great majority of practitioners, in place of giving one large dose, divide it into several, which are given at intervals of one or two hours, during the intermissions.[6]

In Italy, Dr Watson says, the physicians find small doses inadequate, and are in the habit of giving 12, 24, or even 30 grains at a time.[7] I found myself, however, during a protracted residence in the north of Italy, that the general practice consisted in administering repeated small doses. On the other hand, we find a German physician, Dr Pfeufer of Heidelberg, recommending the administration of a single large dose, in preference to the repeated small ones.[8]

In India the practice by repeated small doses has been, and still is, the most general; and I myself shall never forget the look of astonishment with which an apothecary of some standing in the Indian service regarded me, when the "new assistant-surgeon" ordered a scruple dose of quinine. The attention of the profession in India, however, is being now called to the superiority of the treatment by large doses, and principally owing to the advice of the late superintending surgeon Corbyn, who, in a published annual report on European troops,[1] states, that he has long been convinced of the efficacy of this mode of giving quinine, and mentions the results of the experience of several medical officers, to whom he had recommended the practice. Of these, Dr Mackinnon, who had been in the habit of giving half-drachm doses of quinine at the termination of the sweating stage, says, "I have never seen it fail to put a stop to the disease at once." Dr Mactier speaks equally favourably of the practice.

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[1] Lectures, Lond. Med. Gazette, vol. x. [2] Vol. i., p. 943. [3] Vol. i., p. 237. [4] Christison's Dispensatory, p. 774. [5] Lectures, vol. i., p. 766. [6] Diction. de Medecine et Chirurgie, tom. viii., p. 145. [7] Lectures, loc. cit. [8] British and Foreign Med. Chir. Review, April 1850.

On my first arrival in India, I had resolved to put to the test of experiment the practice recommended by my former preceptor, Dr Christison; and I was not a little gratified in afterwards finding, by the published report of Dr Corbyn, that results, equally favourable with my own, had been obtained by other observers. The result of my observations, then, on the 115 cases, goes to prove that the practice most effectual in at once checking the paroxysms of ague, is that of administering one large dose of quinine during the third or sweating stage. The usual dose given was 20 grains in a draught, with a few drops of sulphuric acid to increase the solubility of the salt. Generally this was followed by a few 2-grain doses, twice or thrice a-day, *per precauzione*, as the Italians say; but I believe that this is hardly necessary. In no case, even in those in which there had been violent headache and other symptoms of cerebral congestion during the paroxysm, did I observe any unpleasant symptoms from the physiological action of the drug. Many of the patients complained of slight buzzing sounds in the ears, but I believe that more or less of this symptom is necessary for the sure success of the medicine; at all events, when it occurs, it is a sign that there is no use of pushing the medicine further. In 95 of the 115 cases, this treatment by the one large dose was adopted. In 56 of these 95 cases, or 59 per cent., the paroxysms were at once checked, there being no return after the administration of the single large dose of quinine. In 36 cases, or 37·9 per cent., there was only one paroxysm; and in 3 cases, or 3·15 per cent., two paroxysms, after the quinine. In all the cases, moreover, in which there were any paroxysms subsequent to the administration of the large dose of quinine, these were much milder than the preceding ones, often not occurring, until after the intermission of upwards of 24 hours, or being unaccompanied by rigors in the cold stage; while, in several instances they appeared to be owing to costiveness of the bowels; for it seemed necessary for the

lera, had daily commercial intercourse with the capital; and many of them were seized by the fever on their way home to the interior. Of these, not a few died as they reached the foot of the Cordillera; and though I heard of one who crossed the mountain pass and got as far as *Jauja*, about fifty leagues from the coast, and died of the fever after all, yet I have learnt of no instance where it was conveyed from one to another on the cold table-lands. The *vomito prieto*, or even the epidemic of which it was so marked a symptom, was, if I be allowed the metaphor, a chick that could not break its shell without a more fostering warmth than that of the lofty Sierra.

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[1] Indian Annals of Medical Science, vol. i., October 1853.

In conclusion, I shall briefly notice that at this very time a letter was received in Lima from the Prefect of the distant province of Conchucos which excited deep interest, as it was feared that the same epidemic which filled the capital with mourning had also broken out among the Andes. It appeared that at Llumpa and Masqui, two villages in Conchucos, and situated in warm glens whose waters bend their course eastward to the great basins of the Amazons, there had prevailed since the month of January (1844), an epidemic disease which carried numberless victims in its train; and rapidly spread from north to south until it reached the village of Chacas, situated in a cold and elevated neighbouring district. But the Indians of these parts took the alarm, and put all the affected places under strict quarantine, to prevent the further diffusion of the dreaded malady, the origin of which was assigned to unusual rains; so that the ground becoming swampy, and again acted upon powerfully by solar heat, was believed to have engendered a deadly miasmatic poison.

The symptoms of this inland epidemic, so far as could be collected from non-professional reports, consisted of pain in the bowels, with vomiting and purging—probably cholera.

Medical Notes on the Climate of Burmah, and on the Diseases which have there prevailed among European Troops.

By Charles Murchison, M.D., Physician to the Westminster General Dispensary, and late of the Bengal Medical Service.

(Continued from p. 85.)

In my former paper I made some observations on the geographical and geological position, and on the climate of Prome, mentioned a few particulars connected with the arrival of the 2d Bengal European Regiment at that station in 1853, enumerated the diseases which prevailed in the regiment for some months after its arrival, and brought forward a few of the principal causes to which these diseases might with propriety be attributed. I now, in the fifth place, proceed to give a few more detailed observations on some of these diseases.

The great preponderance of febrile over other diseases has already been pointed out, upwards of 75 per cent. of the admissions into the hospital at headquarters, for the first three months after the arrival of the regiment at Prome, and upwards of 79 per cent. in the detachment sent into the district, being on account of this class of diseases. These fevers may be said to have been of two forms, the intermittent and remittent; for, though a few cases of continued fever were noted, yet, as already mentioned, they were very few in number, and closely approximated in characters to the remittent fevers, in all but the absence of a well marked period of remission. The number of cases of intermittent fever during the three months was 219; of remittent, 105; and of continued, 4; while, of the eighty-six cases of fever occurring in the detachment, just alluded to, seventy-nine were intermittent, and seven, remittent.

I shall, in the first place, make a few observations on the intermittent fevers, and afterwards on the remittent, contrasting the characters of the latter with those of the continued fever, which prevailed in the same regiment at Calcutta during the hot, dry months of May and June of the same year.

I. *Intermittent Fevers.*—Of the 219 cases of intermittent fever admitted into the hospital at Prome, 178, or 81·3 per cent. were quotidian, and 41, or 18·7 per cent. tertian; and of the 79 cases in the detachment 71, or 89 per cent., were quotidian, and the remainder, tertian. It must be observed, however, that a few of the quotidian cases might be cal
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lera, had daily commercial intercourse with the capital; and many of them were seized by the fever on their way home to the interior. Of these, not a few died as they reached the foot of the Cordillera; and though I heard of one who crossed the mountain pass and got as far as *Jauja*, about fifty leagues from the coast, and died of the fever after all, yet I have learnt of no instance where it was conveyed from one to another on the cold table-lands. The *vomito prieto*, or even the epidemic of which it was so marked a symptom, was, if I be allowed the metaphor, a chick that could not break its shell without a more fostering warmth than that of the lofty Sierra.

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2. *Rigors.*—In 19 of the 23 cases, or 82·6 per cent., the paroxysms commenced with rigors, generally severe; in 4 cases there were none, and the cold stage consisted only in a feeling of chilliness over the surface of the body. The proportion of cases in which the rigors were absent, is very similar to what we found to be the case in the quotidian form.

3. *Headache*, generally severe, was an almost constant symptom during the hot stage. In one case only was there none.

4. *Enlargement of the spleen.*—In all the cases which were examined with care, this organ was found more or less enlarged during the paroxysms, but in only one case was this enlargement permanent and attended by much pain.

5. *Nausea and vomiting* were frequent symptoms.

6. *Pulmonary symptoms.*—In three of the cases there was a severe spasmodic cough during the paroxysms, probably owing to the pressure of the enlarged spleen upon the diaphragm. In one case there was slight pharyngitis.

7. *Urinary symptoms.*—In one case there were severe pains, resembling those produced by the passage of a stone from the kidneys down to the bladder—great pain in the region of the kidneys stretching down the thighs, with scanty, high-coloured, scalding urine. No gravel, however, could be detected in the urine.

8. *Administration of the quinine.*—The observations we have already made, on the best mode of administering the quinine in the quotidian intermittents, apply with equal force to the tertian. Here also it was found, that the administration of one large dose at the commencement of the third stage was not only the most efficacious in at once checking the paroxysms, but also the most economical as regards the expenditure of quinine. The truth of this will appear from the following results:—In 18 out of the 23 cases this treatment was followed, and in 14 cases, or 77·7 per cent. the paroxysms were at once checked, while in the remaining 4 cases only one paroxysm followed the administration of the quinine. In all these 4 cases, too, the subsequent paroxysms were very slight; in one case there were no rigors, and in two the want of success from the quinine was probably owing to the bowels remaining very costive. On the other hand, of the remaining 5 cases, in which the quinine was given in repeated small doses during the intermissions, one had three

paroxysms subsequent to the commencement of treatment, another had two, and two had one; in one case only of the 5 were the paroxysms at once checked. As regards the consumption of quinine in the treatment by the large doses, it was found, that the average amount necessary to check the paroxysms was only 25⁵⁄₇ grains, while in the treatment by repeated small doses, the quantity necessary for the same purpose amounted to 37 grains. Here then was a difference of 11 grains in the average amount, according to the two modes of treatment, and we believe that with larger data the contrast would be shown to be still more striking. It may be also observed, that those cases treated by the one large dose, appear on the whole to have been more inveterate than the others, if we may judge of the inveteracy from the number of paroxysms which preceded the treatment. The average number of these in the first 18 cases was 2⅝; in the last 5, only 2.

One case of *double tertian* came under my notice, in which the paroxysms, though daily, commenced at 8 A.M. and 6 P.M. in the alternate days.

The following statements will, I think, demonstrate to all the importance of studying the mode in which quinine may be administered most economically in intermittent fevers. I have taken considerable trouble to learn the amount of quinine annually sent out to our Indian possessions, and have ascertained, that, during the three years 1851, '52, '53, there were sent out to the three presidencies—

3,804 pounds avoirdupois of disulphate of quinine.
12,690 do. do. cinchona bark.
5,050 ounces do. amorphous sulphate of quinine.

Now, putting aside the cinchona and amorphous quinine, we shall suppose that three-fourths of the 3804 pounds of the disulphate were employed for arresting the paroxysms of intermittent fever. Three-fourths of 3804 pounds would be 2853 pounds, or 19,971,000 grains. Now, according to Dr Christison's calculations from the Madras reports, that it requires on an average 36 grains of the disulphate to cure a tropical intermittent, the above quantity might be expected to cure 554,750 cases. But, supposing the average quantity found necessary in each case not to exceed on an average 25 grains, which, as we have seen, was found sufficient in the severe intermittents of Burmah, the same number of cases might be cured by 13,868,750 grains,—thus saving 6,102,250 grains, or 13,948 ounces avoirdupois. Now at the present selling price of quinine in bulk, about nine shillings an ounce, 13,948 ounces would amount in value to £6276, 12s., which sum would have been the difference in the expense

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[3] Cyclopædia of Pract. Med., ii., p. 221.

debility produced in the different cases, by considering the length of time, which elapsed before the patient was able to return to his duty. After his discharge from hospital, each patient was in general excused from duty for two days.

3	Patients were discharged on the	1st	day after last paroxysm.
17	... ...	2d	... ...
21	... ...	3d	... ...
44	... ...	4th	... ...
10	... ...	5th	... ...
12	... ...	6th	... ...
2	... ...	7th	... ...
1	... ...	9th	... ...
1	... ...	10th	... ...
1	... ...	12th	... ...
3	... ...	15th	... ...

Most of the patients were discharged before the fifth day, and all, except eight, before the seventh. The average stay was 4 ^{22} days. Those detained in hospital longer than the usual p^{eri}od, had some organic disease in addition to the fever.

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12. The next column shows the *duration of the paroxysms* in the cases in which this was accurately determined. This was done in 95 out of 115 cases, with the following results—

In 1	case the paroxysm	lasted	2	hours.
2	cases	...	... 3	...
2	...	...	... 4	...
14	...	...	... 5	...
14	...	...	... 6	...
20	...	...	... 7	...
27	...	...	... 8	...
4	...	...	... 9	...
6	...	...	... 10	...
1	...	...	... 12	...
3	...	...	... 14	...
1	...	...	... 16	...

In estimating the duration of the paroxysms, the third stage was scarcely included, but the duration was counted from the commencement of the rigors until the full establishment of perspiration. We thus see that the ordinary duration was 7 or 8 hours; the longest, 16 hours; the shortest, 2; and the average, $7\frac{27}{95}$ hours. This is somewhat under the duration laid down by most writers. With regard to the duration of the paroxysm, Dr Copland, speaking of intermittents generally, remarks, it "varies from 4 to 16 hours, the common length of it being about 6 or 7," while that of the quotidian "continues the longest."[1] Dr Watson says, "The average duration of the paroxysm the quotidian is 10 or 12 hours;"[2] and Dr Jos. Brown says, "It will be very near the truth, to estimate the mean length of the whole paroxysm at 16 hours."[3] But this estimate would appear to be rather above the mark.

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[1] Dr Watson's Lectures, vol. i., p. 765.

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6. *Treatment.*—During the premonitory symptoms, especially when there was great nausea without vomiting, an emetic appeared to give great relief; and in a few cases, indeed, it would seem to have checked the attack altogether. But, generally speaking, the patient was not seen until after the fever was fully established. From what has already been said with regard to the type of the fever, it must be obvious that it was necessary to exercise great caution in having recourse to depletion. All the cases that had been freely bled, exhibited the most aggravated typhoid symptoms, and most of them died. Venesection, therefore, was never put in force after its bad effects were observed in the first few cases, even in the instances of young robust recruits with more or less acute symptoms; for even in them, low, adynamic, typhoid symptoms were sure to supervene after a time; and though the bleeding might give some temporary relief, it was certain to aggravate, if not induce the subsequent typhoid condition. When the headache was very severe, and the pulse full, a few leeches, eight to twenty, were applied to the temples at the commencement of the attack; the hair was cut, or the head shaved, and cold lotions applied to the head, or the cold douche kept up for ten minutes at a time: this last often gave great relief. As soon as possible after the commencement of the attack, a purgative of calomel and jalap, or of colocynth, antimonial powder and calomel, was administered, so as to ensure free motion of the bowels. After this, a saline antimonial mixture was given at repeated intervals, and powders containing two or three grains of calomel and antimonial powder, with the object of promoting the secretions of the skin and liver, both of which were obviously interrupted. When typhoid symptoms showed themselves, stimulants and counter-irritants were had recourse to, such as wine, brandy, carbonate of ammonia, effervescing draughts, sinapisms to the abdomen, feet, and thighs, blisters to the back of the neck and scalp, turpentine enemata, &c. But in the treatment of this fever, as in that of intermittents, quinine was undoubtedly the sheet-anchor. This was generally given in repeated small doses during the remissions; but I think there could be no doubt, that here, as in intermittents, the best way of administering it was in one large dose at the very commencement of a remission. After this treatment was had recourse to at an early stage of the fever, and depletion avoided, not a single

fatal case occurred out of about 80 cases; but during the first fortnight, before this treatment was adopted, and when bleeding was practised rather largely, 5 out of about 25 cases died. Nevertheless, we think it but right to mention, that some practitioners, who had the same means of judging, still advocated copious depletions; but the above facts appear to me, to offer most conclusive evidence against the propriety of such a practice. There can be no doubt that, in the treatment of many of the fevers of tropical countries, depletion is absolutely necessary; but still there can be as little doubt, that in India, as elsewhere, the treatment of fever must vary most materially at different times and in different places; that at one place, and at one time, the very opposite treatment may be required from that which will be necessary at others, and that the prevailing type of the fever must never be lost sight of. Here the prevailing type was indubitably asthenic, and consequently depletion was counter-indicated. The beneficial effects of quinine in these cases confirm, in some measure, the extraordinary results which have been published by Mr Hare, surgeon in the Bengal Medical Service, and which at one time excited considerable sensation among the medical profession in India. The principal feature in Mr Hare's treatment of remittent fever, consisted in the administration of large doses of quinine, from one to two drachms daily. The more severe the fever, the more frequently was the scruple dose administered, till complete cinchonism was produced. He waited for no remission before giving the quinine, not regarding this medicine as an antiperiodic. "Fevers," he says, "destroy life by an accumulation of blood in the veins of the brain and abdominal viscera, and quinine has the power of stopping these accumulations." But if this be the action of quinine, we might expect it to be equally serviceable in the treatment of many of the continued fevers of this country; but in these we have ourselves seen it employed, and signally fail.[1] We believe that it is only in those fevers which are intermittent or remittent, that quinine is of any service; and, therefore, to say that it acts as an antiperiodic, probably expresses its true action as well as our present knowledge admits—an opinion, by the way, which is confirmed by the fact, that quinine is also of service in various other periodic complaints.

II. We now proceed to give a few *illustrative cases* of the above described *remittent fever*.

Case I. *Remittent Fever—Depletion—Typhoid Symptoms—Death.*

Private A. M., æt. 24, admitted into hospital at Prome, November

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[1] See Dr Bennett's cases in Edinburgh Monthly Journal, 1852, vol. i., p. 355.

5, 1853, at 1 P.M. Fever of two days' duration, but for some days previously had complained of listlessness and want of appetite. On admission pulse above 100, and full. Skin hot and dry. Tongue dry, parched, and covered with a whitish fur. Great thirst. Bowels confined. Violent headache.

App. Hirud. xii. temp.
Sumat. mist. sal. diaph. ℥j 2da. q. q. hora.

Vespere.—Much in same state. Great headache. Bowels confined. Hair to be cut close and cold applied.

App. Hirud. xii. temp.
℞. ext. colocynth co. gr. vj. Cal. gr. iij. Pulv. Antim. gr. iv.
M. Div. in Pill. ij. stat. sumend.
Contin. mist. sal. diaph.

Nov. 6.—Did not sleep much, but perspired a little, and this morning appears better. Pulse 88, of moderate strength. Headache and thirst less severe. Only one motion of bowels.

℞ Magnes. Sulph. ℥j. Vin. ant. gutt. xxx.
Aq. ℥iv. M. Ft. haust. st. sumend.
Contin. Mist. sal. diaph. et Lot. frigid. cap.

Vespere.—Pulse has risen again this afternoon to above 100, and headache and thirst have greatly increased, with occasional wandering delirium. Tongue red at edges, with a white fur along centre. Bowels have been freely moved.

App. nuchæ cuc. cr. ad ℥xii.

Nov. 7.—Dosed a little during the night and perspired slightly. This morning skin hot and dry. Pulse 96, rather full but soft. Headache less severe. Great thirst. Bowels open. Is very restless.

Contin. Mist. Sal. Antim. Lot. frigid. cap.
Body to be sponged with cold water. Accidulated drinks.

1 P.M.—Pulse 100, rather full, but soft. Headache not so severe as yesterday. Has been perspiring profusely since sponging of body.

Venæsectio ad ℥xvj. Blood presented no buffy coat.

Vespere.—Less headache. Pulse 120, and weak. Cold, clammy sweats.

Nov. 8.—Passed a quiet night, but has perspired profusely, and is very weak. Skin is still moist and cool. Less thirst. Tongue covered with a light brownish fur. Two or three motions of bowels during night.

Contin. Mist. Sal. diaph. Arrow-root.

Vespere.—Is very weak, and has been very restless all day, with occasional subsultus tendinum. Pulse 100, small, weak. Does not hear well. Complains of no pain, but has hardly slept since admission. Skin warm and moist.

Hab. haust. C. tr. op. gut. xx. vin. ant. gutt. xxx. ex. mist. camph. ℥j.

Nov. 9.—Slept a little after the draught, and appears somewhat refreshed. Complains of no pain, but is still deaf and inattentive to what is passing around. On speaking to him he assumes a foolish expression, with his eyeballs fixed in one position, but puts out his tongue, when asked. Occasional subsultus tendinum. Tongue moist, with a whitish fur. Bowels not moved since yesterday. Is very weak. Pulse 100, small and thready.

App. nuchæ. Emp. vesic. 6 × 4.

Omit. Mist. Sal. Ant. To have sago.

Noon.—Has suddenly become worse. Pulse at wrist almost imperceptible. Extremities cold, clammy, and livid. Lips very livid. Tongue covered with a thick brown fur, and gums and teeth with a large quantity of brownish sordes. Is very restless, and is with difficulty kept in bed. Has occasional delirium, but generally answers rationally when spoken to. Pupils natural. On the backs of his hands and forearms, and also over the upper part of the chest, are a number of circumscribed purple petechiæ, about $\frac{1}{16}$ in. in diameter.

Head shaved, and Tinct. Lyttæ rubbed into scalp. Sinapisms to epigastrium and calves of legs. Enema Terebinth.

Did not rally at all, but continued to sink. Extremities became colder and more livid. About 1 P.M., breathing became laboured, and ultimately stertorous, and twenty minutes after, he expired. He answered rationally up to the commencement of stertorous breathing. The *post-mortem* appearances were these which have already been described. Great congestion of all the abdominal organs. Slight congestion of the arachnoid. Blood dark and fluid. Some blood in the left cavities of the heart, right cavities empty.

Remarks.—Though this and the following case were admitted under my care, I may state that I do not hold myself responsible for their treatment. The above case illustrates, in a lamentable manner, the injurious effects of extreme depletion and withholding of stimulants.

CASE II. *Remittent Fever—Depletion—Typhoid Symptoms—Recovery.*

Private W. G., æt. 28, admitted at Prome, November 5, 1853, with fever of four hours' duration, but had been feeling out of sorts for some days before. Pulse 100, full, but soft. Tongue with a yellowish fur. Great nausea and thirst. Bowels confined. Headache, but not severe.

Hab. Pulv. Ipecac. ℈ c. Ant. Tart. gr. j.

Post. horas ij. haust. sennæ ℥iv.

Mist. sal. Ant. ℥j. 2 ā q. q. hora.

Lot. frigid. capiti.

Nov. 7.—Perspired freely after the emetic, and all yesterday was much better and almost free from fever. This, however, has increased during the night, and the headache is much more severe than it has been yet. Pulse 104, full. Skin hot and dry. Great thirst.

App. cucurbit. cr. nuchae, ad ℥x.

Hab. haust. Magnes. Sulph. ℥j., Vin. Ant. ʒss. ex. aq. ℥iv.

Contin. Mist. sal. Ant. et Lotio. frigid.

Vespere.—Headache continues. Pulse 100 and full. Skin hot and dry.

App. cuc. cr. ad. ℥xii. nuchae.

Nov. 8.—Pain in head greatly relieved, but complains of giddiness. Pulse 120, soft. Great thirst. Tongue with a white fur. Bowels rather confined. Skin cooler.

℞ Ext. Col. Co. et Pil. Hyd. ā ā gr. v. M. Div. in Pill. ij.
St Sumend.

Contin. Mist. Sal. Antim.

Vespere.—Febrile symptoms have increased during the day. Has had no motion of bowels, and complains of pain and tenderness of abdomen.

Sinapism. abdomini. Hab. Ol. Ric. ℥j. c. Tr. Opii gutt. x.

Nov. 9.—Is better again. Pains in abdomen gone, and has no headache, but still complains of giddiness, and is restless. Pulse 100, soft. Tongue with a white fur. Great thirst. Bowels freely moved.

App. nuchae. Emp. Lyttae 6 × 3.

Hab. 3tia q. q. hora. Cal. gr. v. Pulv. Ant. gr. iij. c. Camph. gr. j.

1 P.M.—Headache has increased, and skin is warmer, but moist. Pulse 96, and soft.

Venæsectio ad. ℥xii.

Perspired profusely and was very faint after the bleeding. Blood presented no buffy coat, and did not coagulate firmly.

9 P.M.—Still continues very weak. Pulse 100, very small and weak. Skin moist. Extremities cold and clammy.

Omit. Mist. Sal. Antim.

Hab. Quin. Sulph. gr. x. repet. post horam; et cras mane.
Warm Sago.

Nov. 10.—Slept some, and feels easier. Pulse 100, but very soft and weak. No headache. Skin cool and moist. Feels very weak.

Quin. Sulph. gr. v. 3tia q. q. hora.

Noon.—Quinine was discontinued, as patient was complaining of buzzing sounds in ears. Pulse exceedingly weak and often difficult to count. Extremities cold and clammy. Tongue with a brownish fur.

Ordered warm sago with port-wine (℥ss.) every 2 hours.

Nov. 11.—Pulse has considerably improved, but is still very weak. Tongue dry and brownish at base, but at edges is red and moist. No buzzing in ears. Feels much exhausted from want of sleep.

To continue wine and sago.

℞ Sp. Ammon. aromat. ʒvj. Tinct. Card. Co. ℥j.
Aq. ℥ij. M. Sumat. ℥ss. 3â q. q. hora.

Vespere.—During day was feverish for some hours, and wine was discontinued. Pulse improving.

Hab. Tr. Hyoscy. ʒj. ex. Aq. Menth. ℥j.

Nov. 12.—Did not sleep well. Pulse 96, and has improved considerably in strength. Tongue red and moist at edges, but with a dense brown fur over the centre and tip. Great thirst. Bowels confined.

Ol. Ricini ʒiv. Haust. efferv. c. Tr. Aurant. 2da q. q. hora.

Sago and milk. Port-wine ʒv.

Vespere.—Haust. c. Tr. Hyoscy. ʒj.

Nov. 13.—Slept well, and feels much refreshed. Pulse down to

70, and improving in strength. Less thirst. Tongue as yesterday. Bad taste in mouth. Bowels open. No headache. Is very feeble.

℞ Tinct. Gent. Co. et Tinct. Card. Co. ā ā ℥jss.
Quinæ Sulph. gr.xii. aq. Menth. ℥ix. M.
Sumat ℥j. mane et vesp.

From this period he continued gradually to regain his strength. His tongue grew cleaner. His appetite returned, and on November 28th, he was able to return to his duty.

Remarks.—Here then was another case exemplifying the injurious effects of venesection, which in both cases, it may be observed, was had recourse to at a period when it was certainly counter-indicated by all the symptoms of the case. This case, but for the employment of stimulants, would no doubt have terminated in a similar manner to the former.

CASE III. *Intermittent Fever passing into Remittent—Typhoid Symptoms—Death.*

Private P. M., æt. 22, admitted January 24, 1854, in camp at Taphoon. Young and robust, and but recently arrived from England. Had been much exhausted by the fatigues and privations during the expedition into the Tharawaddy district, already described. Fever of three days' duration, intermittent, quotidian. The first two days he had no rigors, but to-day the fever came on with severe rigors at 11 A.M. Skin hot and dry. Pulse 100, full but soft.

Ordered Mist. Sal. Ant.

Vespere.—Pulv. Ipecac. Co. gr.viij. c. Pulv. Antim. gr. iv.

Jan. 25.—Perspired freely during the night, and this morning is quite free from fever and headache. Pulse slow and soft. Skin warm and clammy. Tongue furred. Bowels costive.

Hab. Pulv. Jal. Co. ʒj. c. Cal. gr. iv. et post horas ij. Quinae Sulph. gr. x.

Vespere.—Has had another paroxysm to-day, preceded by no rigors, but accompanied by great headache. One motion of bowels.

Hab. Pulv. Ipecac. Co. gr. v. Pulv. Antim. gr.iij. Cal. gr.j.

Jan. 26.—Perspired freely last night but slept none, and this morning is still somewhat feverish. Complains of slight headache. Pulse 84, soft. Skin clammy.

Hab. Pulv. Cinch. ʒss. ex. aq. Menth. ℥j.

Vespere.—Had another paroxysm to-day, coming on at 11 A.M. No rigors. Bowels confined.

Hab. Cal. gr. iij. Ext. Col. Co. gr. v. Pulv. Antim. gr. iv. in Pill. ij.
Mist. Sal. Antim.

Jan. 27.—Perspired freely during night, but has slept none, and has been very restless. Pulse 84, small and soft. Slight headache, and pains all over body. Skin clammy.

Tongue furred, two motions of bowels.

Hab. Pulv. Cinch. ʒss. ex Aq. Menth.
Repet. post horas ij.

Vespere.—Another paroxysm came on in the afternoon with violent headache.

Pulv. Antim. gr. v. c. Cal. gr. j.

Jan. 28.—Perspired freely in early part of night, but has been very restless and slept but little. This morning pulse quick, small and soft. Skin dry and rather warm. Tongue parched and furred at base. Great thirst. Bowels open. No headache, but complains greatly of inability to sleep.

Hab. Pulv. Cinch. ʒss et repet. post horas ij. c. Pulv. Op. gr. j. Contin. Mist. Sal Ant. Haust. acidul.

Vespere.—Has continued feverish all day, and slept none.

Hab. Tr. Op. gr. xxv. ex Mist. Camph. ℥j. et repet. post horas iij. nisi dormiat.

Jan. 29.—Slept for some hours after second draught, but has been very restless during the greater part of the night, and still continues so. Complains much of being unable to sleep and feels exhausted. Tongue with a dry brownish fur at base. Pulse 80, very weak. Skin warm and moist.

Hab. st. Tr. Op. ʒss. ex Mist. Camph. ℥ss. arrow-root and port-wine.

Vespere.—Restlessness and heat of skin increased somewhat during the day. Skin is now cool and clammy. Pulse 76, weak. Tongue furred.

Hab. st. Quinæ Sulph. gr. x. ex Sp. Vin. Gallic ℥ss. et aq. ℥ij.

Brandy and warm water every hour.

Jan. 30.—Very restless all night, and did not sleep longer than ten minutes at a time. A good deal of low wandering delirium; pupils are contracted. At present he is lying on his side asleep. Pulse 78, weak.

4 P.M.—Did not sleep long in the morning, but has been very restless all day, and quite sensible. At present he has suddenly fallen into a semi-comatose condition. Breathing laboured. Lies on his back, and does not answer when spoken to, but yet appears to know what is said to him, putting out his tongue when asked to do it. Tongue dry and brown. Skin cool. Pulse quick, but very small and soft. Pupils contracted.

App. Capit. Abras. Emp. Lyttæ.

Enema Terebinth.

To have ℥ss brandy in warm water every half hour.

5½ P.M.—Hab. Omni hora Ammon. Sesquicarb. gr. iij.

Sp. Aeth Sulph. gutt. x. Sp. Vin. Gal. ℥ss. aq. ℥j.

7 P.M.—Sinapisms to soles of feet.

7½ P.M—Sinapisms to abdomen.

These remedies were of no avail. The coma became more profound, and the breathing more laboured, and at last sterterous; while the pulse got weaker, and the skin colder, and about 8½ P.M., he expired. No convulsions before death.

Post-mortem appearances corresponded with those already described. There was less congestion of the abdominal organs, and this was the case before alluded to, as peculiar on account of the veins on the cerebellum being much congested, while those on the cerebrum were normal.

Remarks.—This was an instance of well-marked intermittent fever passing into the remittent form. It is to be observed that this was the only fatal case of remittent fever, in which depletion was not had recourse to, and that it occurred in the expedition into the Tharawaddy district, the fatigues and privations attendant npon which we have already shown to have tended greatly to debilitate the constitution, and render it less able to withstand the onset of disease. Owing also to the supply of quinine in camp being almost exhausted, we were unable to administer it in such doses, and so often as we could have wished.

Case IV.—*Remittent Fever.*—*Cure.*

Private J. E., aet. 21. Robust; only one year since arrival in India. Had five febrile exacerbations in all. Complained of great chilliness all night of November 20, 1853, followed by febrile symptoms on morning of 21st. Pulse 92. Great headache, thirst, &c. Furred tongue. Worst in afternoon, remission towards evening.

Nov. 22.—Second exacerbation.

Nov. 23.—Third exacerbation—very severe, coming on about noon. Pulse 100, full, and rather strong. Skin dry and very hot. Face flushed, great headache, occasional vomiting, pain in stomach. Was now admitted into hospital. Treated with purgatives, saline antimonials, 8 leeches to temples, cold lotions, &c.

Nov. 24.—Fourth exacerbation still more severe. Similar symptoms to yesterday, with delirium, deafness, great restlessness. With difficulty kept in bed. Face flushed. Dorsal decubitus. Pupils dilated, but contractile. Pulse 100, rather full, and of some strength. Is hardly conscious, but puts out his tongue when asked. Tongue dry, and with a white fur. Lips parched.

Abradatur caput. Emp. lyttæ nuchæ. Cold lotions to head, and tepid sponging of body.

Vespere—In a profuse perspiration.

Hab. quinæ sulph. gr. v. 3tia q. q. horâ.

Nov. 25.—Very weak. Pulse 100, and weak and small. Tongue with a slight brownish fur at base. Vomiting. Pupils dilated. Had a fifth exacerbation this evening from 9 to 12 p.m., but slight. After this the febrile symptoms gradually disappeared, and the quinine was continued for two or three days. Under tonic treatment he gradually regained his strength, and on Dec. 12 returned to his duty.

Remarks.—Here was a case in which the patient's habit of body and the symptoms, in the first instance, seemed to demand depletion more than any of the preceding, but which did well without it. Even in this case, the acute symptoms in the outset soon merged into those of an adynamic character. The tepid sponging of the body appeared to be of great service in inducing perspiration.

Case V.—*Remittent Fever.*—*Cure.*

Private Jos. C., aet. 24. Robust, and only one year in India. Was seized with fever early in the morning of Nov. 20, which had continued for nearly 36 hours, when he was admitted into hospital, on the evening of the 21st, in a high fever, complaining of violent headache and great thirst. Face flushed. Pulse 100, full, and of moderate strength. Tongue with a white fur. Bowels confined. Hair was cut, and cold lotions applied to head, and 20 leeches to the temples. A purgative of calomel and extract. coloc., and repeated small doses of calomel and Dover's powders. Low diet and accidulated drinks.

Nov. 22.—Had a slight remission during night, but skin continues warm and dry, and pulse quick.

23.—Had a second exacerbation yesterday, with remission in night-time. Skin cooler, pulse 96, and soft.

Quinæ sulph. gr. ij. 2 ā. g. g. hora.

24.—Had a third exacerbation yesterday, with high fever, violent headache, and restless delirium. Urine dark, of high sp. gy.; not coagulable. Experienced great relief from the cold douche overhead. Had a remission during night, which continues.

Contin. quin. sulph., ut antea.

25.—Remission continued all day yesterday; but had a fourth exacerbation during the night, of short duration and less severe.

Contin. quin. sulph.

26.—Had a fifth exacerbation last night, which has subsided towards morning in copious perspirations. Urine not coagulable. Is very feeble. After this had no return of the fever. Gradually regained his appetite and strength, and returned to his duty on the 8th of December.

Case VI.—*Remittent Fever.—Cure.*

Corporal J. W., aet. 28. Had only two exacerbations in all: one on the 3d and the other on the 4th of December 1853, with remissions towards evening. The exacerbations presented the usual characters, and during the remissions there was slight perspiration. On admission he had a purgative, and saline antimonials during the exacerbations. At the commencement of the first remission he got 10 grains of quinine, and 2 grains every third hour afterwards. The same treatment was followed in the remission after the second paroxysm.

Remarks.—This case was one of many others, which well exemplified the beneficial effects of quinine, given in the manner already recommended.

Case VII.—*Remittent Fever.—Cure.*

Private D. A., aet. 26. Had three exacerbations in all. The first came on with a feeling of great chilliness on the evening of November 7th, 1853, and continued for twenty-eight hours. Had a remission all the 9th; but in the evening had a second exacerbation, lasting till 4 a.m. of the 10th. Had a third exacerbation in the afternoon of the 10th, lasting till the morning of the 11th.

The characters of the exacerbations and remissions corresponded with those already described in the preceding cases, the only thing remarkable being that the patient always complained of severe aching pains in the joints during the exacerbations, which disappeared during the remissions.

The treatment consisted in purgatives, saline antimonials, cold to the head, &c., with quinine, 5 grains every three hours, commencing with the second remission.

Remarks.—This case also exemplified the benefit to be derived from quinine. On comparing the four last of these cases with the two first, the conviction must be forced upon every one, that copious sanguinous depletions were counter-indicated in those fevers.

III. We shall now proceed, as briefly as possible, to contrast with the remittent fever we have just been considering, the characters of the *continued fever* which prevailed in the same regiment at Calcutta, during the previous hot, dry, season. The cases of continued fever in the regiment were almost confined to the hot, dry months of April, May, and first part of June. They gradually became less frequent after the commencement of the rains, about the middle of June.

1. *Type.*—This fever was essentially sthenic in type, and attended by a greatly increased vascular action, with a tendency to local determinations of blood, and almost invariably demanded copious depletion. These characters were, as might be expected, best marked in young, full-blooded recruits, who were also the most liable to the disease. The fever, in fact, presented most of the characters of that which has been described under the term of "inflammatory fever," or the synocha of Cullen, and, in most respects, corresponded with what is known as the seasoning fever of Europeans in tropical climates.

2. *Causes.*—There is but little probability that malaria had anything to do with the production of this fever. Many malarious fevers, no doubt, often put on an inflammatory, continued form; but the peculiarity of this fever was, that it was confined to the hot, dry season, under circumstances the least favourable to the generation of malaria. The exciting cause of the fever appeared to be the excessive heat, the thermometer ranging daily from 92° to 100° Fahr. in the shade; and during the night seldom falling below 84°. This was no doubt aggravated by a number of individuals occupying the same apartments in the barracks, and thereby increasing the closeness and temperature of the air. Incautious exposure to the direct rays of the sun was the cause to which, in many cases, the disease was referred by the patients themselves. The great heat might act by rarefying the atmosphere, and thereby impeding respiration and the due oxygenation of the blood; while in the case of direct exposure, it might produce local determinations to the brain. Among

the causes which appeared to *predispose* to the disease may be mentioned early age, a robust, plethoric habit of body, the use, or rather the abuse of spirituous liquors, a costive condition of the bowels, and great fatigue, from the excessive drilling to which the men were subjected; while chills, individual acts of intemperance, &c., appeared to act as determining exciting causes.

3. *Premonitory Symptoms.*—In some few cases the patients complained of listlessness, lassitude, want of appetite, furred tongue, costive bowels, and slight headache for one or several days before the commencement of the attack; but in many cases, especially those which appeared to originate from exposure to the direct rays of the sun, the attack came on suddenly without any premonitory symptoms.

4. *Symptoms during the fever.*—This commenced generally with a feeling of chilliness, sometimes so slight as scarcely to attract attention, at other times almost amounting to rigors. In some cases there was great nausea, and even vomiting. After this the skin became hot, often burning, and dry, and the face flushed. The pulse rose to from 100 to 120, and was full and hard. There was violent headache, with throbbing of the temples, ringing in the ears, and muscæ volitantes. The tongue and lips were dry and parched, the former with a thick, white, or yellowish fur. There was a complete loss of appetite, and the patients complained of a bad taste in the mouth and unquenchable thirst. There was often tenderness in the hepatic region, with increased dulness on percussion. The bowels were confined. The urine was scanty, often scalding, dark coloured, of high sp. gy., acid reaction, depositing on cooling crystals of lithic acid, and not coagulable by heat and nitric acid. There was great restlessness, pervigilium, and, after a time, dorsal decubitus, acute raging delirium, contracted pupil, insensibility, lividity, coma. The blood drawn separated entirely, on standing, into serum and coagulum, the latter often buffed.

The above symptoms, which were always most severe during the heat of the day, generally reached their climax about the seventh or ninth day, at which period they either began to amend, or death soon ensued. In the fatal cases, death was preceded by acute delirium, coma, dyspnœa, stertor, &c. In those which recovered, the amendment was generally ushered in by a copious critical perspiration. After this the pulse fell, the skin became cooler, and remained moist. The urine became more copious and paler, and deposited abundance of lithates; the tongue became cleaner, the thirst diminished, and strength was gradually regained.

5. *Post-mortem appearances.*—The following morbid appearances presented themselves after death :—

A. *Head.*—There was great congestion of the veins on the surface of the brain, and of the sinuses at its base. The pia mater was also congested with sometimes minute extravasations. Occasionally there was opacity of the arachoid, with considerable subarachnoid effusion. The quantity of fluid in the ventricles varied.

B. *Thorax.*—The lungs were very much congested. The right cavities of the heart were full of blood, forming a firm coagulum ; the left were comparatively empty.

C. *Abdomen.*—The abdominal organs were much congested, especially the liver and spleen.

D. *Body.*—There was great lividity of the lips, extremities, and all the dependent portions of the body.

6. *Treatment.*—Copious depletion at the commencement of the attack was almost invariably necessary. In the young and full blooded, this was practised both by venesection and the application of leeches to the temples; in those who had been weakened by a longer residence in India, by leeches alone. The other remedial measures employed were purgatives, such as calomel and jalap, or colocynth, calomel, and antimonial powder; powders of calomel, and antimonial powder, saline antimonial mixtures, acid drinks, low diet, the removal of the hair from the head, and the application of refrigerating lotions. When delirium and coma supervened, the head was shaved, and a blister applied to the scalp or nape of the neck, a sinapism to the epigastrium, and a turpentine enema administered.

Copious bleeding was no doubt the principal remedy to be depended on in this fever, and the great relief which it afforded in many cases to all the distressing symptoms was really very striking. In order, however, that it might be effectual, it was necessary that it should be practised at the commencement of the attack; if delayed till the vital powers had become exhausted, it appeared to cause more mischief than good.

One cannot fail to be struck with the great differences between this fever and the remittent fever which afterwards prevailed in the same regiment at Prome. The former was a continued fever, the latter a remittent; the former was sthenic, the latter, asthenic; the latter was a malarious fever, the former most prevalent at the season least favourable to the generation of malaria; the former was benefited by depletion, the latter proved perfectly intolerant of it.

Among the other diseases which prevailed at Prome, may be mentioned:—

1. *Diarrhœa and Dysentery.*—After fevers, the most frequent diseases in the regiment were diarrhœa and dysentery, and the latter may be said to have been the principal cause of the great mortality among our troops in Burmah. But in Burmah, as, indeed, in India generally, these diseases were far more prevalent during the rainy season than at other periods. During the three months, November, December, and January, immediately succeeding the cessation of the rains, there were admitted into the hospital of the 2d Europeans at Prome 17 cases of diarrhœa, and 21 cases of dysentery, and out of the detachment, which remained for six weeks in the surrounding country, 12 cases of diarrhœa were admitted, and 3 of dysentery. The number of cases appeared to diminish as the season became drier, but again greatly increased after the commencement of the rains. In a medical report of H. M. 80th Regt. in Burmah, we find that in 12 months, out of a mean strength of 487, 228 cases of dysentery were admitted into hospital, and 15 of diarrhœa, or exactly one-half of the mean strength were admitted with one or other of these two diseases. From the proportions in the latter instance it seems probable that many cases of diarrhœa have been entered as dysentery, and indeed in some cases it was found a difficult matter to decide between cases of chronic dysentery and diarrhœa. In the cases which we have mentioned from the 2d Fusiliers, all those were entered as diarrhœa, in which the history and symptoms did not afford pretty certain evidence of the existence of inflammatory action or ulceration of the bowel, in which there were merely frequent loose feculent stool, without blood or mucus, and without abdominal tenderness or any great amount of tenesums. Many of the cases of diarrhœa proved very obstinate, and if at all neglected, they were very apt to lapse into a chronic form of dysentery, and in this way terminate fatally. Both these diseases exhibited a marked tendency to adynamic symptoms, and bore depletion with great difficulty. One of the cases of diarrhœa terminated fatally, and in rather a remarkable manner. The diarrhœa, which had lasted for some days, had been checked by medicine, and the patient was apparently getting better, when, from no apparent cause, symptoms of extreme prostration suddenly showed themselves, the pulse became slow, small and weak, and at length intermittent, the extremities cold, &c., and after two or three hours, death preceded by slight convulsions, terminated the scene. On *post-mortem* examination, the only abnormal appearances found, were considerable congestion of the abdominal organs, especially the small intestine, the lower half of which presented a deep livid hue. The brain, and the heart's

texture and valves, were all normal. All the cavities of the heart contained blood.

As might have been expected from the debilitating influences to which the troops were subjected, the cases of dysentery in Burmah were very apt to become *asthenic*, and were remarkably fatal; yet we observed no well-marked cases of what has been described as "Scorbutic Dysentery," which was so prevalent and fatal in the first Burmese war. Of the twenty-four cases above mentioned, six, or one-fourth, died, and of those cases that recovered, in many the cure was only temporary, the disease exhibiting a strong tendency to return. I believe it was the opinion of most medical officers who had served in Burmah, that after an individual had had an attack of Burmese dysentery, the only chance he had of a permanent recovery was by leaving the country; as long as he remained in it, he was constantly liable to a relapse, and that relapse probably fatal. In addition to these six fatal cases of dysentery, one of the officers was seized with the disease immediately on his arrival at Rangoon, and was unable to proceed on to Prome. He recovered under treatment, and was ordered back to Calcutta by a Medical Committee, but while waiting for a steamer, he had a relapse which proved fatal. The disease in this case, presented a feature which is not unfrequent in Burmese dysentery, namely, copious discharges of pure blood from the rectum, producing very great and rapid prostration. "Hemorrhagic Dysentery," indeed, has been called by some medical officers "the disease peculiar to Burmah," but as far as my experience went, this extreme loss of blood presented itself only in a minority of cases. In only one of the other six cases did it take place. In this case, the disease was found after death to be principally seated in the rectum, this part of the bowel being one mass of ulceration, which, at some parts, almost perforated the walls of the gut, and whose surface was covered with adherent black clots of blood. The excessive hemorrhage both in this and other cases, was no doubt owing to the rapid ulceration having laid open one or more vessels of considerable size. The disease was occasionally remarkably rapid in its course, sometimes proving fatal on the 6th or 7th day; while, at the same time, the local symptoms were comparatively slight; and it was observed, that a few days' illness sufficed to produce the most extensive ulceration of the large intestine. In one case, which proved fatal on the 7th or 8th day, the colon was found in one mass of ulceration from the coecum to the rectum. No positive evidence could be adduced of the disease being contagious, though the other patients in hospital often made use of the same night stool as the dysentery cases. The causes to which the disease might be attributed, have already been considered, but we would here more particularly mention one, to which in many cases the origin of the disease appeared attributable, and which certainly, in all cases, greatly aggravated it, when already existing. What I allude to are the chills, which the men experienced from the cold, damp morning air, and from the little protection offered by their thin walled hospitals and barracks. It was observed in all the cases, that the symptoms underwent an aggravation from three to six o'clock every morning, and the patients themselves attributed these aggravations, and no doubt with perfect justice, to their having " caught a chill."

All the fatal cases were made the subject of careful *post-mortem* examination, and the following observations may not be without interest :—In all cases the disease was confined to the large intestine. Dr Bleeker of Batavia, who has had most extensive experience in tropical dysentery, and whose recent paper on the subject[1] is one of the most valuable that has been written, observes, that, in scarcely 2 per cent. of his cases, did he find the disease pass the boundary between the small and large intestine. Again, in the large intestine itself, the disease was found to be either generally diffused, or limited to certain localities. In the latter case the localities were two in number, the rectum and the coecum, especially the former. It might exist in one or other, or in both of these, and in those cases in which it was more generally diffused, it was always most advanced in these two localities. In no instance was the disease limited to, or farthest advanced in, the ascending or transverse colon. The ulcers were generally found in various stages of progress. This was particularly noticed in one of the cases, and the appearances which presented themselves in this case seem of considerable importance in reference to the interesting subject of the mode of origin of ulcers in the intestines. The case was that already mentioned, as presenting the hemorrhagic form, and in which the ulceration was found to be principally seated in the rectum. As already mentioned, the ulceration in the rectum was far advanced, at some points almost amounting to perforation; but in the upper parts of the bowel, especially the ascending colon, the disease was observed in much earlier stages. The various stages, by which the disease commenced and progressed, appeared to be somewhat as follows :—1. The earliest stages observed appeared to consist in the formation of a number of vascular spots, produced by finely injected ramifying capillaries. These spots presented a more or less circular outline, and varied in diameter from $\frac{1}{10}$th to upwards of one inch in diameter. The larger ones were less vascular in the centre than towards the circumference. 2. Similar spots, with a minute ulcer in the centre. This ulcer, in many instances, was not larger than a pin's head, but presented a remarkably circular and regular outline, impressing one very forcibly with the conviction, that it had been produced by the bursting of a minute pustule or vesicle. I may observe, however, that I was unable to detect any such pustule or vesicle in the centre of any of the vascular patches; but the subject well merits investigation, as it is not improbable that in the mucous membranes, as well as in the skin, there are pustular and other diseases, which may end in ulceration. Dr Bleeker describes the mode of origin of the ulcers as considerably different. He says, a mass of fibrinous exudation, varying in form, is first deposited in the coats of the intestine; that this then softens in the centre, while the superimposed mucous membrane decays, and is broken, establishing an ulcer. 3. These ulcers appeared gradually to increase in size, preserving their even outline, but passing from a circular to an elliptical form, and becoming more or less indurated at their edges. This induration was very great in some of the more chronic cases, approaching in consistence to that of cartilage. 4. The ulcers then appeared to extend still more, until contiguous ones communicated with each other by their edges, forming one large mass of ulceration, with its irregular surface covered with portions of sloughing membrane and adherent clots of blood.

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[1] Translated from the Dutch by Theodore Cauter, M.D., Indian Annals of Medical Science, vol. i.

In none of the fatal cases of dysentery, was there observed any complication of disease of the liver beyond congestion. Of 24 fatal cases of dysentery occurring at Rangoon in H.M. 80th Regiment, Mr Taylor found abscess of the liver in 4. Again, Mr Stewart, surgeon, of H.M. 18th Royal Irish, says, that in three-fourths of the fatal cases of dysentery in his regiment in Burmah, the liver was found more or less implicated, from simple engorgement to abscess.[1]

As regards the treatment, considering the asthenic character of the disease, it is obvious that copious depletion was counter-indicated. General bleeding was never resorted to, and even leeches were applied more sparingly than is generally found to be necessary in India. In the hemorrhagic cases, in which there was generally severe tenesmus, great relief was always derived, from the application of a few leeches round the anus, but nothing appeared to be of much avail in restraining the hemorrhage. After the removal of the leeches, the treatment consisted in the application of warm fomentations to the abdomen, and the administration of repeated doses of ipecacuan, either combined with opium in the form of Dover's powder, with extract of gentian in the form of Twining's pill, or in mucilage of gum arabic, as recommended to me by Dr Mudge of the Madras army. This medicine, whether or not in virtue of its action as a sedative, certainly appeared to be of more service than any other. Laxatives, and emollient and anodyne enemata, were also found of great service. When there were any symptoms of hepatic congestion, blue pill was combined with the ipecacuan, and it was generally observed, that a decided amendment followed the affection of the system by the mercury, if this could be obtained, which, however, was often difficult. In the more chronic stages blisters were applied, and the various mineral and vegetable astringents administered, both by mouth and rectum, but in some cases the whole category of these was tried with but little benefit. When ulceration was fully established, as evidenced by the peculiar flaky appearance of the stools, the nitrate of silver, given in repeated half-grain doses, seemed to be productive of more benefit than anything else which was tried. In cases in which the disease proved very obstinate, or was constantly relapsing, the patient's only chance of safety consisted in a complete change of climate.

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[1] Indian Annals of Medical Science, vol. i.

I may here briefly allude to a treatment of dysentery which has acquired an Indian notoriety, viz., that of Mr Hare, a surgeon in the Bengal medical service. This gentleman regards dysentery as a malarious disease, and recommends the administration of large scruple doses of quinine. This, with enemata of water thrown up into the colon by means of a long tube, constitutes the principal part of his treatment. His success, according to his own accounts, has been most astonishing; but other practitioners have not met with similar results, and, generally, the practice is not in good odour among our medical brethren in India.[1]

2. *Cholera.*—Fortunately this disease did not show itself in an epidemic form, among the 2d Europeans on their arrival in Burmah, and up to the end of January only four sporadic cases occurred. According to the classification of the Medical Board of Calcutta, one of these cases belonged to the *cholera biliosa*, and three to the *cholera spasmodica.* Three of the cases recovered; one died, not from the immediate effects of the disease itself, but, being an elderly man, he sunk

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[1] Mr Hare on "Tropical Fever and Dysentery," Indian Annals of Medical Science, Oct. 1853; also a Refutation of Mr Hare's Treatment, a pamphlet by Dr John Macpherson, Bengal army; also *Edin. Medical and Surgical Journal.* July 1854.

three or four days after the attack, under a double pneumonia. The treatment of cholera in India appears to be as diversified and unsatisfactory as it is in Europe. As the castor oil treatment has been the rage with some practitioners in England, so in India there are practitioners who are in the habit of giving a much stronger purgative—croton oil. Its success, however, like that of the castor oil, has not been such as to recommend it to the profession. Other practitioners, such as Mr Barry of the Bengal service, recommended strongly the nitrate of silver.[1] Copious bloodletting, saline enemata, carbonate of soda, inhalations of ether and chloroform, quinine, pills of ox gall, &c., are among the other remedial measures to which we are instructed to have recourse. Calomel, however, appears to be the medicine in which the best Indian practitioners place the most reliance, and perhaps the best way of administering it is in combination with stimulants, such as capsicum and carbonate of ammonia. Dr James Anderson, of the Bengal army, one of the most experienced and talented of our profession in India, recommends the abstraction of a few ounces of blood from the arm, in the first onset of the disease, in robust patients, with the administration of a mustard emetic. This treatment, followed up by repeated small doses of calomel with opium, and the early use of stimulants, both externally and internally, I have myself seen very successful in his practice.

3. *Hepatitis.* During the three months of November, December, and January, five patients were admitted into hospital on account of this disease, though many of the patients, admitted for other complaints, were found to be labouring under chronic forms of the disease. Three of the above five cases were acute; two, chronic. One of the former died, the patient being thirty-three years of age, having served about twelve years in India, and long led a very dissipated life. On dissection, the liver was found to be enormously enlarged, and the whole of the right lobe was converted into an immense abscess, containing upwards of four quarts of pus. The walls of the abscess, in many places, presented a very ragged appearance, owing to numerous adherent portions of lymph and disintegrating hepatic tissue. At two places the walls were exceedingly thin, one situated immediately below the margin of the right ribs, and the other posteriorly near the mesial fissure. The pus had a reddish tinge, and, on microscopic examination, was found to contain pus corpuscles, a quantity of oil globules and granules, and hepatic cells, undergoing disintegration. An obstinate diarrhœa had been one of the most harassing symptoms in this case, but no traces of ulceration were found either in the small or large intestine.

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[1] Indian Annals of Medical Science, vol. i., pp. 116, 451.

4. *Dyspepsia*, in various forms, sometimes associated with hepatic derangement, and sometimes independent of this, was, as might be expected, a very common complaint.

5. *Pulmonary diseases* were very rare. Two cases of pneumonia were observed, one already alluded to, as supervening on an attack of cholera and proving fatal, and the other contracted from exposure to the cold, damp, night air, during the expedition into the district, and also proving fatal. In both cases the pneumonia was double, and in the latter there were found distinct traces of old tubercles at the apices of both lungs. It is worthy of remark, that this morbid appearance is extremely common in the lungs of European troops, dying in India of diseases, with which it has no connection. In the numerous *post-mortem* examinations I witnessed in India, I made a point of always examining the apices of the lungs, and, in certainly the majority of cases, I found small cretaceous nodules, cicatrices, or other traces of old tubercles. From this it appears probable, that the disease is often arrested in its infancy, by the removal of the patient to a warm climate.

6. *Tape-worm* (*tænia solium.*)—I allude to this case merely from the treatment employed, for the disease was not contracted in Burmah, the patient having suffered from it for many years before he left India. He came into hospital at Prome, complaining of emaciation, loss of appetite, nausea, gnawing pains in the stomach and bowels, and debility. He had been treated repeatedly with turpentine, pomegranate, and strong purgatives, but with only very temporary benefit. Having no other anthelmintics at hand, I ordered a number of pods of the Indian Cowitch (*Mucuna prurita*), a common weed in the jungles, to be collected, the hairs to be shaken off, and a tea-spoonful of these, mixed up with a little syrup and tincture of cardamoms, to be given three times a day on an empty stomach. After the fourth dose, a full dose of castor oil was administered, which brought away a number of joints of the *tænia solium.* The treatment was continued for a few days, and on the fourth day the patient said he felt sure he had got rid of the worm, and that he was more comfortable than he had been for years. Three months after, he was quite well, and was gaining flesh. It is to be observed, that the worm was discharged dead, and in detached pieces, and the action of the medicine was in all probability purely mechanical, the animals dying of the excessive torture produced by the hairs. The hairs of the West Indian cowitch (*Mucuna pruriens*) have been found serviceable in lumbricus and ascarides, but in these instances the worms have been expelled alive. Dr Christison, however, remarks that the remedy "has little effect in tape-worm."[1]

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[1] Dispensatory, 2d. Ed., p. 650.

7. *Rheumatic affections* were not uncommon. During the three months there were admitted into hospital five cases of acute, and three of chronic rheumatism, and slighter rheumatic affections were not uncommon in many not admitted. The prevalence of this disease was no doubt owing to exposure to the cold, damp, night air, while on sentry-duty.

8. *Venereal diseases.*—On perusing the tables, we must have been struck with the very few cases of venereal diseases, which are so common in our regimental hospitals in other parts of the world. During the three months only one case of gonorrhœa, and one of primary syphilis were admitted into hospital. The truth is, the Burmese themselves are not addicted to promiscuous sexual intercourse, and venereal diseases, except in their seaport towns, were almost unknown among them, before our arrival in the country. It is to be feared the same cannot be said now.

9. *Ulcers.*—Several of the men suffered from extensive ulcers, resulting from scratching mosquito bites.

10. *Cephalalgia*, without fever, was a very common complaint during the prevalence of the mid-day sun, especially in the young and robust.

11. *Epilepsy.*—One man was seized with epileptic fits, for the first time, shortly after his arrival at Prome. Mineral tonics, the introduction of a seton into the neck, and regulated diet, checked the fits while he remained in hospital; but they always returned when he was allowed to resume his duty.

12. *Apoplexy.*—Two fatal cases occurred; one at Prome, the other at Taphoon.

13. *Delirium tremens.*—On their first arrival at Prome the troops did not drink to excess, and during the first three months there was only one case of this complaint; but, after a time, ennui drove them to the bottle, and in the month of February, there were three cases of delirium tremens in hospital at one time. The liquors to which they had recourse, were the native 'toddy,' the fermented juice of the Tari palm, a very common tree at Prome, and shamshoo, a very intoxicating liquid, obtained from the distillation of rice.

14. *Debility.*—As already observed, the climate of Burmah appeared to have a most depressing effect upon the constitution. Most diseases exhibited a tendency to adynamic or typhoid symptoms, and independently of disease, the constitution became enfeebled, and many of the men found themselves unable to perform their ordinary duties.

I have now brought these desultory observations to a conclusion, and have only to regret that they are not more perfect. The peculiar circumstances under which many of them were made, when marching through the jungles of Burmah, must plead my excuse.

ART. III.—*Short Account of the Cases treated in the Cholera Hospital, Surgeon Square, during the late Epidemic.* By J. WARBURTON BEGBIE, M.D., Fellow of the Royal College of Physicians, Edinburgh.

(Read to the Medico-Chirurgical Society, January 3, 1855.)

In 1848 Edinburgh acquired the by no means enviable distinction of being "the first part of the United Kingdom attacked by cholera."[1] In 1853, the disease, after an absence of little more than three years, made its re-appearance at Newcastle-on-Tyne on the 30th day of August, and before the close of the following month cholera had again broken out in this city. But, though for the third time very early in the list of places in the United Kingdom in which cholera appeared, we have nevertheless to congratulate ourselves, that the duration of the epidemic in Edinburgh, on the last occasion, has not been lengthened, nor its effects very fatal. The hospital in Surgeon Square, which, as the members of the Society are aware, was, during the epidemic of 1848–49, set apart for the reception of cholera patients, in September 1853 was again opened for the same purpose, and at the request of the Sanitary Committee of the City Parochial Board, I undertook the duties of its visiting physician. From the circumstance of my having occupied that position, I have felt it incumbent upon me to offer to the Society a short account of the cases treated in the hospital. To this subject the following remarks will be confined, as I am neither desirous nor able to give an account of the epidemic as it prevailed throughout the city generally. During the late epidemic the hospital in Surgeon Square was originally opened on the 16th of September 1853; it was temporarily closed about the commencement of June 1854, to be again opened on the 24th of August. It was finally closed a little more than a fortnight ago; the last admission being on the 30th of November, and the last dismissal on the 11th of December ultimo. During the whole period there were admitted, in all, 243 patients, of which number only 45 were brought to the hospital during the earlier period, viz., from September 1853 to May 1854, and 198 during the period which intervened between the end of August, and the commencement of December 1854. The idea that the whole of the cases which occurred were somewhat equally distributed over these two distinct periods must not, however, be entertained, the Sanitary Committee having very wisely on both occasions, kept the hospital open and retained the services of a medical staff, for what appeared a safe period, during which, probably for weeks there were not admitted more than one or two cases at most. The proper impression will be conveyed, when I mention, that during the first period, as many patients were admitted during October and November as during all the other months, and that from October 3d to 30th inclusive, 15 out of the whole 45 were admitted. Again, during the latter period, of the 198, no less than 168 were admitted during September and October, and in September alone 97. From the 22d to the 26th of September the average of the cases under treatment in the hospital daily exceeded 25; on the 23d of September there were 28 cases in hospital. Placing the numbers occurring during the two periods together, we find that the experience of the hospital has been roughly as follows :—Total number admitted, 243—of these, *males*, 97 ; *females* 145 ; *total recoveries*, 126 ; *total deaths*, 117. *Deaths among males*, 43 ; *deaths among females*, 74 ; *recoveries among males*, 54 ; *recoveries among females*, 72. The total recoveries thus exceed the total deaths by 9, while the mortality among female patients exceeds by 2 the recoveries ; and among the male patients falls short of the recoveries by 11. Among these cases, however, it must be remembered, that not a few are included which were not cases of genuine cholera ; thus, the number of recoveries will shortly be found to suffer a fall, and we shall also have to diminish the fatal catalogue by reason of a few deaths having occurred from other diseases than cholera, for during an epidemic it is by no means uncommon for patients labouring under severe disease in many different forms to be found occupying beds in a cholera hospital. Accordingly, we shall have to notice two fatal cases of peritonitis and more than one death from other causes, dysentery, &c. In several instances the age, owing sometimes to the condition of the patient, was not ascertained. The following table gives in decennial periods the ages of the cholera patients which were correctly noted—

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[1] Dr Sutherland's report to the Board of Health, page 123.

	Males.	Females.	Total.
Under 10 years,	4	4	8
From 10 to 20 years,	13	9	22
„ 20 to 30 „	21	30	51
„ 30 to 40 „	20	24	44
„ 40 to 50 „	20	27	47
„ 50 to 60 „	8	16	24

	Males.	Females.	Total.
From 60 to 70 years	7	11	18
Above 70 „	1	3	4
	94	124	218[1]

As regards the *habits* of the patients before admission, it was ascertained that of the little more than 200 adults, 95 were notoriously intemperate, and, with the exception of 51 who were reputed sober, there was reason to believe that the habits of the remainder were more or less irregular. Forty-seven adults were removed to the hospital from houses dirty and overcrowded, not a few of these had, in addition, darkness and dampness to afflict them. Many were in an enfeebled state of body, in privation, wanting the necessaries of life, alike as regards food and clothing. Several found in open stairs were brought to the hospital by the police; one woman had for many years slept, save at intervals when a tenant of the gaol—without shelter of any kind,—she recovered. Of 53, however, it was affirmed that the houses from which they were brought were clean, and of other 20 that their houses were *tolerably* clean and comfortable.

The following are the streets and parts of the city, new and old, from which patients affected with genuine cholera were brought to the hospital, the asterisk indicating the localities from which several cases were brought:—

* Canongate
* High Street
* Lawnmarket
* Grassmarket
* West Port
Trunk's Close
Fleshmarket Close
Chalmers' Close
Bailie Fyfe's Close
St Leonard Street
Hastie's Close
* Candlemaker Row
Baxter's Close
* Hume's Close
Blackfriars' Wynd
Pleasance
Drummond Street
Castle Hill
St Mary's Wynd
Big Jack's Close
James' Court
Alison Square
Advocate's Close
Anchor Close
* Leith Wynd
Hyndeford's Close
Fountain Close
Monteith's Close
Stevenlaw's Close
* Crosscauseway
Greenside
* Hope's Land
Potterrow
Craig's Close
Cholera Hospital, Surgeon Square
Burnett's Close
Horse Wynd
Fishmarket Close
Maconochie's Close
Calton Street
Duncan Street
William Street
India Street
Rose Street
Little King Street
* Canonmills
Thistle Street
Jamaica Street
Water of Leith
Poplar Lane, Leith
Canal Court, Leith
* Holyrood[2]

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[1] A glance at this table will show the uniformity, as regards age, which exists in the seizure of males and females. Of the 124 females, 81 were between the ages of 20 and 50; and of the 94 males, 61 were within the same periods of life. Adult age seems the one at which attacks of cholera are most frequent; for, keeping all the circumstances in view necessary in such an inquiry, it does appear that neither in youth nor in advanced age is the disease so common.

[2] The influence of locality in relation to cholera, and of elevation, (a point

One case was brought from the prison and one from the Magdalene Asylum. The former was a remarkable case in more than one particular. He was the only prisoner who on this occasion suffered from the disease—had been confined for twenty days before he was attacked, and had he survived one other day, his period of imprisonment would have terminated. The elements are certainly wanting in this case to constitute any very probable theory of its having originated by contagion. It was, further, a very rapid case—from seizure till death only thirteen hours intervened. On looking over the table of localities from which patients were removed to the Surgeon Square Hospital during the epidemic of 1848-49, as given by Dr William Robertson in his very interesting paper, I have been much struck by their very nearly exact sameness with those I have just read. In some particulars, indeed, the correspondence is most remarkable, the same localities in Leith are noted, and the parts of the High Street, Canongate, Cowgate, Lawnmarket, and West Port of Edinburgh are almost identical. Only one case of genuine cholera originated in the hospital during the epidemic. It occurred during the first period, and was that of an elderly female of very intemperate habits and broken-down constitution. She was connected with the laundry, and had no occasion to enter the wards, nor, so far as I am aware, had she ever done so; but she washed during several successive days the clothes of patients who were ill, or had died of the disease; a means, very potent, I have been led to believe, in the communication of the disease, and one which it becomes medical men to be very careful in advising regarding. During this period diarrhœa was common among both medical men and attendants. During the latter period, when, too, there were many more patients in the hospital, there originated no case of genuine cholera, and diarrhœa was not frequent among doctors or nurses. Dr Robertson's experience in 1848-49, contrasts with this—five nurses contracted the disease, and three of the five died.

Of the 243 cases, the following were *certainly* not cholera, and these I enumerate in order to indicate the cases which are sometimes admitted to a cholera hospital:—

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which has been most ably handled by Mr Farr), I did not possess the means to investigate, but these are subjects of great interest, and well-deserving attention; it is to be hoped that in Edinburgh they will not be lost sight of; indeed it were well, that those who are charged with the supervision of districts, should keep them in view, as bearing on the origin and progress of all epidemic diseases.

1.	A female,	aged 20,	tubercular peritonitis,	Died.
2.	A man,	aged 30,	acute peritonitis,	Died.
3.	A woman,	aged 70,	severe gastralgia,	Relieved.
4. 5. 6. 7. 8. 9. 10. 11.	Cases of intoxication brought by the police during the night, having either vomiting or diarrhœa, and not unfrequently cramps.			Recovered.
12.	A male child,	aged 2,	hydrocephalus,	Died.
13.	A woman,	aged 20,	delirium tremens,	Removed to Infirmary.
14.	A man,	aged 40,	epilepsy,	Dismissed.
15.	A female,	aged 25,	hysteria,	Dismissed.
16.	A female,	aged 50,	dysentery,	Recovered.
17.	A man,	aged 60,	dysentery,	Died.

The 243 cases are thus diminished by 15, and the 226 are further to be reduced by those cases which were *evidently* more or less severe bilious diarrhœa; this done, there will still remain a few *doubtful cases* among the genuine cases of cholera; these, however, it is impossible, from obvious reasons to separate, and all we can decidedly affirm of them is, that to the best of our knowledge, though they did not pass into the second stage of the disease, collapse at one time appeared more or less imminent. Of cases of diarrhœa which were easily cured, and never presented any threatening symptom of cholera, there were 18, and this further reduction leaves 208 cases of genuine cholera. Of 68 cases of genuine cholera, in which the condition of the patient on admission was accurately noted, 42 were in a state of either *profound* or *decided* collapse, or in a state *approaching* to collapse, and 26 were affected either with the characteristic discharges from the bowels and stomach, or, these having been arrested, with some other combination of symptoms distinctive of the disease. Of the 42, 28 were females and 14 males. Of the 26, 12 were females and 14 males. Of the 42 admitted in collapse, 36 died and only 6 recovered. Of the 26 who had not passed into collapse, 4 died and 22 recovered. The former experience does not appear to me to militate against the success of hospital treatment, so much as against the removal of patients to hospital in whom the disease has advanced to the stage of distinct collapse. The latter statement appears to favour the propriety of removal in the case of patients whose symptoms, though unequivocal, have not advanced to the stage of collapse.

A few other facts of interest may be noticed here. Of

35 fatal cases whose period of first seizure appeared to have been accurately noted, in 25 it occurred during the night between 9.30 P.M., and 5.30 A.M. In 10 during the day. In 14 of the 35, admission to the hospital took place during the night, and in 21 during the day. In these 35 fatal cases, the period which elapsed between admission and death was as follows:—

In 1	4 hours.
In 1	5½ hours.
In 3	12 hours.
In 2	13 hours.
In 3	14 hours.
In 1	15 hours.
In 2	18 hours.
In 1	21 hours.
In 11	1 day.
In 1	27 hours.
In 3	2 days.
In 1	3 days.
In 1	4 days.
In 2	5 days.
In 1	7 days.
In 1	8 days.
35	

Thus, fourteen survived less than twenty-four hours, and only ten a longer period than one day.

In connection with some of these cases, there are a few points of particular interest. It has been stated that "pregnancy is a predisposing condition to a fatal attack of the epidemic." Whether pregnancy does or does not predispose to cholera, I do not feel prepared to answer; but it is abundantly evident, that the risk of the disease proving fatal is increased by the co-existence of that condition. Of patients in this state brought into the hospital, the following facts were noted:—

1. A female, æt. 30, seven months pregnant; died in consecutive fever after being nearly four days in hospital; did not miscarry.
2. A female, æt. 26, six months pregnant; died in consecutive fever on the fourth day in hospital; miscarried two days before death.
3. A female, æt. 30; five months pregnant; died in consecutive fever on the third day in hospital; miscarried day before death,

It is to be remarked that all three passed from collapse into the stage of fever, that in that state two of the three miscarried, and that all died.

Two nursing mothers were admitted; one recovered, the other died. The facts observed in the hospital appeared to confirm the opinion that the weak and the cachectic are not peculiarly liable to cholera. None of the persons labouring under other forms of disease who were admitted contracted cholera, and, with few exceptions, the cholera patients themselves appeared to be free from lesions of importance. Neither did the existence of previous extensive disease seem, as might be at first imagined, to cause a fatal complication; at least, two patients, both females, suffering from advanced phthisis, struggled through very severe attacks of cholera.

One of the points, to the elucidation of which my endeavours were always directed in the hospital was, whether or not cases of cholera are preceded by a premonitory diarrhœa. Certainly, in most of the cases, this question received an affirmative confirmation; still there were not wanting cases in which the most careful inquiry failed to elicit any evidence of such affection. In regard to the statements either of the patients themselves, or of the friends who accompanied them to the hospital, it must be borne in mind that an habitual state of looseness of the bowels is by no means uncommon among the lower orders—a circumstance which may explain the little attention being given in the first instance to a slight or even decided increase of it. Of fifty-three cases in which I imagined that a pretty near arrival at truth was made, there were twenty-one in which there existed no evidence of premonitory symptoms, and thirty-two in which there was certain evidence of their occurrence.

Of these thirty-two,—

Six had diarrhœa for some hours.
Six ... ... one day.
Three ... ... two days.
Four ... ... three days.
Two ... ... four days.
Two ... ... five days.
Two ... ... six days.
One ... ... seven days.
Three ... ... eight days.
One ... ... a few days.
One had sicknesss for two days.
One had vomiting and cramps for four days.

Other cases there were, in which careful examination failed to elicit any evidence of the occurrence of premonitory symptoms. One of the most remarkable of these was that of the lad from the gaol, previously alluded to. At 11 P.M. he was seized with a feeling of sinking, having up to that time been quite well; had only one stool, and shortly afterwards passed into collapse, in which state he died within thirteen hours from his first seizure.[1] For days previously his bowels had been regular, and during the whole course of his fatal illness he had only one stool. There were not wanting other cases in which the *small* number and quantity of the discharges from the bowels bore a marked contrast to the sunken condition of the patient; in all such, however, it was easily ascertained that the matters lodged to a very considerable amount in the intestines. In not a few, in whom diarrhœa had not occurred for several hours, a copious discharge took place shortly after death.

Bloody stools were observed in several cases; in almost all these the patients were old. This symptom was invariably a very alarming one, and no patient recovered on the last occasion who had suffered from it. In regard to the occurrence of *consecutive fever*—in almost all the cases of recovery from severe collapse, the patients became affected with febrile symptoms; in but few cases, however (compared to what occurred in the previous epidemic, and with the accounts I have read of the experience of other practitioners), did these symptoms run so high as to deserve the name of consecutive fever; or did the condition of the patient become again imperilled. Of thirteen cases in which a severe form of consecutive fever did supervene, eight died and five recovered. Of these seven were males and six females. Of the eight deaths four were in complete coma, three of the four with complete suppression of urine. One from whose bladder urine in considerable amount had been drawn off for three days before death; and notwithstanding the tendency to coma, had continued to increase. Three patients died after recovery from collapse, more from acute pulmonary affection—in two bronchitis, in one pneumonia—than from cholera. Twenty-seven deaths, nineteen of females and eight of males, took place in profound collapse; two others, a man and a woman, both in collapse, perished suddenly. Both had become violently delirious, and had been with great difficulty retained in bed: in a moment the delirium appeared to cease; both became quite sensible, but both were perfectly blind, and continued so till death, which did not occur in either for some hours.

The appearances of the body after death, and, in particular, the occurrence of the peculiar movements, "due to the coincidence of contraction in different muscles," were just such as have been frequently observed and recorded. I have no particular observations to offer on the morbid appearances found on dissection of the fatal cases. During the first period, the bodies of nearly all the cases proving fatal were examined; the appearances found confirmed the now generally received opinions in regard to the condition of the alimentary canal, namely, the prominence of the intestinal glands, both aggregate and solitary, especially the latter, in the lower part of the ileum, the distension of the gall-bladder with bile, the absence of urine from the bladder, the dry state of most of the serous membranes, and existence of ecchymotic spots on some of these. I must here record my grateful sense of the kindness of my accomplished friend, Dr Haldane, pathologist to the Royal Infirmary, under whose immediate superintendence nearly all the dissections were conducted. During the latter and longer period of the hospital being open, we were unable to avail ourselves, on account of certain sufficiently good reasons, of the privilege we enjoyed in the former time, in having the use of the *post-mortem* theatre of the Royal Infirmary. The dissections were therefore performed in the Cholera Hospital itself, being, however, limited, by the wish of the Sanitary Committee, to those cases in which permission was given by the friends.

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[1] For this fact I am dependent not merely on the testimony of the patient, but on that of the medical officer of the prison, Dr Simson.

The remaining observations I have to make concern treatment. When the desperate condition of many of our patieuts on admission is held in remembrance, it will be seen that, in a considerable proportion of cases, our efforts were necessarily confined to relieving the sufferings of the victims; in very few with but little hope of anything further—in most with no such hope at all.[1] Even in the most desperate cases, however, their reception into a warm and comfortable ward, with a nurse ready and anxious to administer to any want (and I cannot speak too highly of the faithful manner in which the nurses of the hospital discharged their onerous duties), and those appliances at hand by which we can relieve, though we fail to cure, I have just reason to believe that the hospital proved a blessing. In treatment, some general principles regulated our procedure. In every case we endeavoured to make the poor sufferer as comfortable as possible,—and here I may, once for all, remark, in the words of another, "It is notorious, that for this disease the best remedy is an indefatigable and skilful nurse." To the truth of this observation all who have had any experience of cholera will bear testimony. *Warmth*, by various different means, was applied; by the aid of the ordinary hot bottles; by the adaptation of a gas-lamp to the bed, the heated air being conducted through a tube under a cradle, over which the bedclothes were arranged; an apparatus for which we were indebted to the ingenuity of Mr Smith, of the City Parochial Establishment. We found few patients who were capable of enduring the application of the heated air after this plan for a longer period than twenty minutes. By packing in the hot damp sheet, after the fashion of Dr William Gairdner, of the efficiency of which, in cases of early collapse, I entertain a high opinion; by the hot bath, which, in early collapse, and particularly if the patient be young, and easily carried, is a most valuable adjuvant to every kind of treatment. By *friction*, we endeavoured to allay the cramps, which in many cases were excessive, and of themselves, in some, endangered the life of the patients. This was done either with the dry hand by an attendant, or with some stimulant or anodyne liniment, of which turpentine and tincture of soap and opium were the chief ingredients. By friction along the course of the spine, diligently performed for nearly an hour at a time, I have seen reaction brought about, warmth and pulse restored, when, too, the application of heat alone had altogether failed. I found no means, however, so successful in controlling the violence of the cramps as one I have now to mention, and for which I have to thank a distinguished member of this society, Dr Wyse—namely, the application of the ordinary *tourniquet*, or *tourniquets*, over the limb or limbs chiefly affected.[1] The relief I have seen afforded from this simple means has been very great. The tourniquets require to be tightly screwed down; and seeing that, as a general rule, the patients are unable to bear their application for more than ten minutes to a quarter of an hour at a time, their removal and re-application is invariably required before sufficient relief is obtained. I can scarcely say too much in favour of this expedient in several cases, where the violence of the cramps in the limbs was so great as to threaten life, I have regarded the employment of the tourniquets as eminently conducing to the recovery of the patients. The exhibition of *stimulants* was, in many cases, along with some of the means just mentioned, really all that remained for us to do. I have become quite satisfied, that when the exhibition of stimulants is required, and it often is in the more hopeful class of cases, they should be administered very cautiously, and in small quantities. There is no doubt, that the tendency to the occurrence of fever after recovery from collapse is much increased by the exhibition of stimulants in large quantities during the stage of collapse. I confess the temptation to give large quantities of brandy, or wine, or ammonia, in one or other form (which is in reality safer), is sometimes very strong—irresistible, it would appear; but experience of such cases has tended to prove, that it is even doubtful if the recovery from collapse is secured by them; while if that does happily occur, the tendency to fever with cerebral symptoms is undoubtedly increased. What even more than the liberal exhibition of stimulants, during collapse, leads to this untoward result, is the habitual indulgence in spirituous liquors. I have never seen a drunkard recover from cholera without passing through a marked stage of the post-febrile excitement.[1]

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[1] Of not a few of such, it may be affirmed, that had they been visited at home on the first occurrence of symptoms, the result would have been different. The system of "house to house visitation," which has been fairly tested by Dr Sutherland and others, appears to me one of the most powerful means we possess of combating the disease. It is a plan which, in the event of another visitation of the disease, should be organised in all large towns. (See Dr Sutherland's Report, formerly referred to).

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[1] Dr Wyse, I am aware, is inclined (and from experience) to attribute more decided effects to the employment of the tourniquets than the mere arrestment of cramps. His theory is, that, by the pressure of the tourniquet on the femoral and brachial arteries, the blood is shut off from the extremities; and the circulation being confined to the viscera, reaction is more easily brought about. I can answer for the pressure over the arteries being quite unnecessary for the removal of cramps; all that is required is firm pressure over the body of the limbs. There are very evident objections to Dr Wyse's theory; but any suggestion in practice from him, more especially when the best effects have been observed to follow, must always command our attentive consideration.

There is no doubt that the suppression of urine, and particularly the non-elimination of urea[2] has to do with the development of this febrile condition, but it is not alone its cause. Several patients in the hospital had passed urine in abundance before it occurred, and continued to secrete it, with a due amount of urea, while the state of fever lasted. Two of these, notwithstanding, died in coma. In such cases we must of necessity look for the retention in the blood of some other poison than urea.[3]

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[1] It is very common to hear Indian practitioners contrast the disease, as seen in India, with that witnessed in this country; and in no respect do they look upon this contrast as better marked than in the occurrence of the so-called consecutive fever in the cholera of the latter. That the prevalent use, or rather abuse of alcoholic drinks, has much to do with this result, we do not entertain any doubt. Even on the continent of Europe the occurrence of consecutive fever, in cases of cholera, seems to have been much rarer than in Great Britain. On this point see *Cases of Cholera*, collected at Paris, by James Jackson junior. Boston: 1832.

[2] The chief morbid characters of the urine in cholera are the presence of albumen, and a very great diminution in the proportion of urea.

[3] Probably not an ingredient of the urine at all, but most likely of the bile.

Before quitting the subject of the suppression of urine in cholera, I may mention, that in some cases in the hospital we had every reason to believe that the abstraction of blood from the loins by cupping, or that dry cupping over the same region, did good, and appeared to hurry on the secretion of urine. I scarcely know whether to attribute any good effects to the exhibition of diuretics. We gave such remedies a fair trial in the appropriate stage of the disease; and of all these we were inclined to attach most remedial virtue to the acetate of potash in scruple doses; largely diluted and frequently repeated, it never caused nausea or sickness. In all cases recovering from collapse I have practised and counselled the employment of the catheter;[1] in most cases, a little dirty urine will be thus removed, while in three or four I have been agreeably astonished to find urine to the amount of six, eight, and even twelve ounces flow through the instrument, the consequent relief to the patient being evidently great—in all such the bladder was firmly contracted on its contents.

Without attempting further subdivision of remedies into classes, I shall merely indicate those from which in our experience in the hospital in Surgeon Square we imagined benefit to accrue. For the arrestment of urgent vomiting no remedy seemed more effectual than the *mustard emetic.* A table-spoonful of mustard carefully mixed in a small pint of warm water was the dose we generally administered—it was occasionally repeated. The immediate effects were distressing enough, but as a general rule, and more particularly when the mustard was retained for a few minutes, the result was most beneficial. All other remedies, even *ice,* a most grateful boon to the patient, have fallen short of the mustard. In less severe vomiting, *ice, bismuth, naphtha, chloric ether,* a few drops of *chloroform, calomel,* in scruple doses, and *sinapisms,* have all appeared to do a certain amount of good. Some cases, I need scarcely add, have resisted all these means, and the establishment of severe blisters over the epigastrium as well. One poor man who survived in collapse for nearly three days, had the most urgent vomiting I ever witnessed. He had been fearfully intemperate, and whisky for which he loudly called, and which in small quantities was allowed him, was the only thing which his rebellious stomach would for more than a few seconds retain. For the control of the *severe diarrhœa* in such cases as those formerly noticed, we were in the habit of prescribing the ordinary *lead and opium pill*, in which I placed very great confidence. *Opium*, too, uncombined, may in such cases be given freely,—in an after stage scarcely so freely, with safety; at least, symptoms of the same class, as stimulants appear to increase, opium appears to cause. The treatment of diarrhœa by *sulphuric acid*, I did not find so successful in hospital practice as I had at one time hoped.

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[1] Upon this point Dr George Burrows writes, "The catheter was sometimes introduced, and urine drawn off, although its presence was not suspected."—*Dr Gull's Report,* p. 219.

When the stools are characteristic of cholera, the great aim of treatment, while undoubtedly to arrest or diminish the exhaustive drain upon the system, is also to alter the character of those peculiar discharges—indeed the latter is an element of the former; hence the folly and danger of making the latter the only object, and by purgation with castor-oil, or any other means, hurrying our patients to their end. It is not my desire to uphold any plan of treatment as superior to all others. We are unfortunately so ignorant of any very successful plan as to make such a course alike inexpedient and unprofessional. When, therefore, I name the exhibition of calomel in small doses, sometimes combined with a small quantity of opium, more frequently without,[1] I would not appeal to the successful results of the practice so much as to the clinical observation of the individual cases. It is right, however, that I should state, that in twenty-seven cases of cholera, in all of which collapse to a greater or less extent occurred, there resulted an equal division of recoveries and deaths.[2] In all these the plan of treatment was rigidly adhered to, and only interfered with to the extent of the employment of stimulants in those cases where a fatal termination appeared to threaten. Calomel in scruple, in ten and in five grain doses, with greater or less proportion of opium, was exhibited in other cases. Three out of the ten recoveries under the first-mentioned plan were salivated severely; the treatment in these was in force regularly for sixty hours. So far as careful observation of these cases went, fatal as well as recovered, we attributed good effects to the calomel.[3]

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[1] One grain of calomel, with or without one-eighth of a grain of opium, every hour.

[2] Dr Ayre, who has expressed very great confidence in the efficacy of calomel, given in two-grain doses every five minutes, records 365 deaths out of 725 unequivocal cases treated with calomel.—(*Dr Gull's Report*, p. 176.)

I have a very great objection (founded on observation of the very disagreeable and often lamentable results) to giving medicines too frequently in cholera. True, a great deal may be done *about* a cholera patient—as, for example, by friction—but the exhibition of medicine every five or ten minutes is undoubtedly very exhausting to the patient, at a time, too, when the conservation of all his strength is required.

[3] If the pathological view indicated in regard to the retention of the bile in the gall-bladder and its absence from the bowels be correct, (for notwithstanding what

In the latter, the restoration of bile to the stools appeared to be uniformly quickened, and before urine in any quantity was secreted, its manifestation appeared critical. In some of the fatal cases the stools had become bilious before death,—in two, bloody. We have alluded to the condition of the gall-bladder after death, and, more especially, to the fact of its being filled with bile,—a circumstance which undoubtedly militates to a certain extent against the supposition that the non-elimination of bile has any very important part to play in the pathology of cholera. It is, however, just possible that a hurried secretion of this fluid may take place in the first hours of the disease (or, when cholera supervenes on diarrhœa, an increased secretion of bile having we know occurred); that the gall-bladder being distended, and the passage of bile into the intestines in some manner interfered with, its non-elimination then takes place until the disease is arrested and overcome. However this may be, the presence of bile in the stools is even more than the presence of urine in the bladder (which, too, I have twice in cases of cholera, both rapidly fatal in collapse, found distended with urine evidently pre-secreted but retained),—an indication of a critical beneficial change. In several cases of severe diarrhœa (choleraic diarrhœa), we found benefit from the persesqui-nitrate of iron, in doses of twenty and thirty drops every hour or two hours: the same remedy was useless in genuine cholera. Dr Cappie has employed the tincture of the muriate of iron in the same circumstances, with, I believe, rather better results. When bile has been restored to the stools, it is well in most cases to keep up the action of the liver and bowels by the substitution for the calomel of some other suitable medicine,—the former, if continued, causing salivation, probably an unnecessary evil. Rhubarb, in ten grain doses, with a double quantity of the bi-carbonate of soda or potash, given once, twice, or thrice daily, were those generally used.

Venesection was practised in two cases when the occurrence of head symptoms after recovery from collapse was threatened.[1] One recovered; the other passed into coma,

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has been said as to its existence in very minute quantity in the stools—in most instances it is entirely absent),—then it follows that the employment of calomel in cholera rests not merely on empirical grounds, but that, from its well-known action on the secerning power of the liver, calomel does fulfil a really important indication.

[1] These were the only two cases in which the lancet was employed; for notwithstanding the strong advocacy for the treatment of cholera by bleeding, the results of the practice, even in the hands of the best informed and most observant, has not been such as to recommend it. Mr Hamilton Bell strongly

and died. *The saline injection of the veins*—for which, after Dr Robertson's experience in the hospital I possessed no favour—was performed in three cases by Dr Asher and Mr Falconer (according to Dr Robertson's formula). All the patients died—two within twelve hours after the operation; while the third, in whose case it was practised three times, survived for two days. The *injection of warm-water* into the cellular tissue in various parts of the body, repeatedly tried, did no good. *Galvanism,* carefully employed in all the various stages of the disease, appeared more hurtful than useful. The *saline powders of Dr Stevens,* when retained, which was not often the case, appeared inefficient,—a philosophical plan of treatment in theory, I do think, but one from which it is singularly difficult to obtain any satisfactory result in practice.

In regard to the cholera hospital in Surgeon Square, Dr Sutherland, in his very able and interesting Report to the General Board of Health on the epidemic of 1848–49, has remarked, "that it was by far the best establishment of the kind in any of the districts under my inspection,"—no small meed of praise, when it is remembered that Dr Sutherland's "inspection" extended to Glasgow, Manchester, Liverpool, Sheffield, and Hull, besides numerous other towns and districts in England and Scotland. It is to me a great gratification to be able to state in this meeting of the profession, that the Sanitary Committee of the Board of Managers of the City Parish, on the late as on the former occasion, most faithfully and efficiently carried out the duties entrusted to them, in providing not only hospital accommodation (in which they were aided by the Managers of the Royal Infirmary), but also every needful appliance and remedial means which ingenuity could suggest or devise.[1] I cannot close without bearing a willing testimony to the faithful and excellent manner in which all the gentlemen with whose assistance I was favoured performed their arduous and most important labours. I truly feel that much of the relief which the hospital in Surgeon Square afforded to the suffering poor was due to them.[2]

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advises bloodletting, but the successes even of his practice is not encouraging. In the Castlehill hospital at Edinburgh, in 1832—

44 cases in the first stage were bled—	7 died.
6 verging on collapse, .	all died.
9 in collapse, . .	7 died.

[1] To Mr Smith, the energetic and benevolent Governor of the City Poor House, all who had to do with the hospital—physician and patient alike—were much indebted.

[2] These gentlemen were, Dr Lauder Lindsay, Dr J. M'C. Cowan, Dr Asher, Mr Falconer, Mr Clarke, and Mr Wilson.

I have purposely refrained from noticing the spread of the disease throughout the city generally, chiefly on account of my inability to do that subject justice, and further, because we may reasonably look to the various district medical officers in connection with the different boards for the most accurate and complete information. One thing, however, I feel bound to notice, because my position as hospital physician gave me an excellent opportunity of witnessing these, namely, the excessive labours which the gentlemen to whom I have just alluded underwent. I do believe that the profession generally entertains a very inadequate idea of the duties performed by district medical officers during epidemics of cholera. I have some right to speak of the indefatigable, disinterested, and generous manner in which, during several months, the visitation of cholera patients was daily and nightly performed by the gentlemen connected with the City Parish. It is proper that the Society should have an independent testimony to the zeal with which they were always animated, the more so that one of their number,[1] and a member of this Society, in the very midst of his labours, fell a victim to the disease.

ART. IV.—*New Method of Treating Neuralgia by the direct application of Opiates to the Painful Points.* By ALEXANDER WOOD, M.D., F.R.C.P., Lecturer on Practice of Medicine.

An immense improvement was effected in our treatment of neuralgic affections, when M. Valleix directed attention to the fact, that while, on the one hand, the superficial nerves of the body are of all others the ones most commonly affected with this disease, there are some points of their course in which it is much more liable to be seated than in others, although, in these, no structural alterations can be discovered to account for this liability. These points are usually more or less morbidly sensible to pressure, even in the intervals between the attacks of the sharp lancinating intermittent pain. A very slight touch in these situations is often sufficient to excite acute suffering; in other cases, however, even firm pressure is borne without any complaint. The points in the course of any nerve which are thus liable to be the seat of tenderness are, according to Valleix:—

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[1] Dr John Mackay, in whom, to a most amiable character and an excellent knowledge of his profession, was added an enthusiastic desire to dispense its benefits to the suffering poor, and who, by reason of his unwearied labours, there is too much reason to fear, fell an easy prey to cholera.

1. The place of emergence of the nervous trunk.
2. The point where a nervous twig traverses the muscles to ramify on the integuments.
3. The point where the terminal branches of a nerve expand in the integuments.
4. The point where nervous trunks become superficial during their course.

It is perhaps scarcely necessary to remark that all these points are precisely those where the nerve tends towards the surface, and therefore where, of course, it is the most amenable to local treatment.

Acting on the result of this observation, M. Valleix introduced a plan of treatment, which, as an external remedy, I have largely employed ever since my attention was first directed to his work in 1842.

It consists in the application of a succession of small blisters *over the points* in the course of the nerves which are painful on pressure. Valleix does not recommend, as a general rule, the application of morphia endermically, but suggests that it may be attempted with advantage in some cases. I have almost invariably employed an ointment containing morphia to dress the blistered surface, and have been accustomed to ascribe much of the benefit of the treatment to this. In some cases, I have seen relief follow the application of an ointment containing strychnine to the blistered surface, but this must be used with great caution, as very disagreeable results often ensue from its use.

It has frequently occurred to me, however, that a more direct application of the narcotic to the affected nerve, or to its immediate neighbourhood, would be attended with corresponding advantage, and as the painful points so frequently correspond with those in which the nerve becomes superficial, I thought this might perhaps be accomplished. In pursuit of this object, I have made several attempts to introduce morphia directly by means of acupuncture needles and otherwise, but without success.

Having occasion, however, about the end of 1853, to endeavour to remove a nævus by injection with the acid solution of perchloride of iron, I procured one of the elegant little syringes, constructed for this purpose by Mr Ferguson of Giltspur Street, London. While using this instrument for the nævus, it occurred to me that it might supply the means of bringing some narcotic to bear more directly than I had hitherto been able to accomplish on the affected nerve in neuralgia. I resolved to make the attempt, and did not long lack opportunity.

Miss ——, an old lady, who had long laboured under gastric and nervous symptoms, had suffered severely for four days from cervico-brachial neuralgia. This lady had the idiosyncrasy of not being able to take opium. Of this she had warned me many years before, when she first came under my care, and I consequently never prescribed it for her; however, once, when she was seen with me by the late Dr J. H. Davidson, he, disbelieving her former experience, prescribed opium, with the effect of bringing on a severe fainting fit.

The narration of her case may date from November 26th. She had not been able to sleep for the three previous nights from the violence of the neuralgic pain, and was quite exhausted with severe suffering. The usual internal remedies, with the exception of opium, had been tried, but without the least alleviation of her agony. Under these circumstances, I resolved to put in practice the plan which I had so long revolved in my mind.

Accordingly, on November 28th, I visited her at 10 P.M. to give the opiate the benefit of the night. Having ascertained that the most tender spot was the post clavicular point of Valleix, I inserted the syringe within the angle formed by the clavicle and acromion, and injected twenty drops of a solution of muriate of morphia, of a strength about double that of the officinal preparation.

In about ten minutes after the withdrawal of the syringe the patient began to complain of giddiness and confusion of ideas; in half an hour the pain had subsided, and I left her in the anticipation of a refreshing sleep.

I visited her again about 11 A.M. on the 29th; was a little annoyed to find that she had never wakened; the breathing also was somewhat deep, and she was roused with difficulty. Under the use of somewhat energetic stimuli, however, these symptoms disappeared, and from that time to this the neuralgia has not returned.

CASE II.—Mrs ——, aged 30, of a gouty family, four years married, no family, short, and plump habit, very pale, menstruation scanty and painful, countenance anxious, urine high coloured, suffers much from flatulence and indigestion, tongue loaded, pulse 98.

After exposure to cold and damp, was seized with shivering and pain in the loins on the 9th December 1853. I saw her on the 13th. The pain in the back had then subsided, but the whole region of the left hip was tender, a distinct painful point was felt near the posterior superior spinous process of the ilium, and another farther down, about the middle of the thigh.

Twelve leeches were directed to be applied as nearly as possible in the course of the sciatic nerve, their use to be assiduously followed by that of warm fomentations, and one of the following powders to be taken every sixth hour.

℞ Pulv. Doveri, gr. v.
Pulv. Colchici, gr. iv.
Hydrarg. c. creta, gr. ii.
M. Ft. pulv.

Next day (14th December) she was considerably relieved; had enjoyed a short but refreshing sleep; the bowels had acted three times freely, the motions being very dark and offensive; thought herself much weakened by the bleeding and purging; directed to continue the powders.

December 15.—Tongue cleaner, gums swollen and spongy, coppery taste in mouth, tenderness of hip gone, the pain otherwise much the same as on the preceding day; feels sleepless and uncomfortable; bowels have not again acted.

Stop the powders; take two turpentine capsules three times a-day; drink plentifully of linseed tea with gum dissolved in it.

December 16.—Feels very uncomfortable, the turpentine has produced severe dysuria, pulse 104, tongue coated, the pain in the hip has not returned, but the pains are more severe and lancinating in the middle of the thigh and round the head of the fibula.

The syringe was introduced at the painful spot in the middle of the thigh, and 10 drops of Battley's sedative solution were injected without any perceptible effect but that of slight smarting at the seat of the puncture. Two hours afterwards the pain ceased, and the patient fell into a deep sleep, from which she woke entirely free from pain in the thigh or leg, but suffering slightly in the region of the malleolus externus.

December 18.—The pain in the malleolus is very severe, and is much aggravated by pressure; the pain in the hip and thigh is entirely gone.

The injection repeated in the malleolar region. This operation was followed by no perceptible effect; but in about four hours the pain began to abate, and ceased altogether in about eight hours from the injection.

The state of the patient's general health required some attention; she is now much better, and her sufferings at the menstrual period are diminished, but she has had two slight returns of the sciatica, for which, at her own request, she has been treated by the opiate injections.

CASE III.—Mrs ——, æt. about 50, widow, plethoric habits, hysterical temperament, has suffered since the cessation of the menstrual flux about 10 years ago, from various anomalous symptoms, of a nervous kind, indicating great spinal irritation. Her liver is enlarged, and her heart is often functionally disordered. She has had severe attacks at different times of visceralgia, and has often suffered from neuralgia, chiefly of the dorso-intercostal and lumbo-abdominal varieties. Her very full habit, as also the cause from which these symptoms obviously arose, coupled with the evidence of cerebral congestion, with which her attacks were frequently accompanied, as also the fact of her pulse being at these times full and firm, her skin hot, and her urine high-coloured, had led me to treat them by cupping, purging, antimonials and salines. Opiates had no effect in alleviating her sufferings, and belladonna and aconite affected too powerfully her nervous system without abating her suffering.

I first tried the syringe with her on the 19th of April 1854, when she was suffering from a severe attack of lumbo-abdominal neuralgia.

The syringe was inserted in the *lumbar point*, a little to the outside of the vertebræ, and 25 drops of Battley's solution were injected.

She had an easier night, and the pain, though somewhat better next morning, was by no means removed, and the following day it returned with such severity that her former treatment had to be resorted to with decided relief. She has certainly suffered less since the operation.

CASE IV.—A married female, aged 23, in the lower rank of life, consulted me May 2, 1854. Had suffered three months previously from a miscarriage which had weakened her much; her countenance is exsanguine, lips nearly colourless, tongue and gums white. A loud bruit is audible over the heart and in the carotids. Has also had a great deal of mental distress.

About three weeks ago came from the country on the outside of a coach, and sat on a very damp cushion. Next day felt as if one leg was longer than the other; she had much pain in the lower part of the abdomen, in which she thought she felt a large painful ball. Pressure on the spinous processes of her dorsal vertebræ gives no pain; but a painful spot is distinctly to be detected a little to the left side. Os uteri open and flabby, very tender on pressure; surface velvety, much mucous discharge.

The following medicine was ordered, with nourishing food:

R Citratis Ferri ʒii.
Syrupi ℥ss.
Aquæ cassiæ ℥ v. ss.
m̄
Sumat coch., amp. ter. indies.

May 16.—Somewhat improved in appearance, pain not abated; 25 drops of Battley's solution were injected into the painful point. About an hour afterwards was seized with violent vomiting, with shivering and severe constitutional disturbance; pain not abated.

May 17.—Vomiting has ceased, but returns with every attempt to swallow, pain much worse, no sleep, tongue loaded, bowels costive. Ordered to use ice freely, and to take a drop of Fleming's tincture of aconite every third hour for four times.

May 18.—Stomach much better, but the pain in the back is not abated. Ordered friction with the Tinctura saponis c. opio.

May 19.—Pain which was easier during the day, became much more severe at night, and she had an opiate by the advice of a friend. Severe vomiting followed its use, and it had no effect either in allaying the pain or in procuring sleep.

I recommended her to persevere with the iron for some time, but have lost sight of her.

CASE V.—Mr ———, after much exposure to wet, consulted me on the 4th June 1854, on account of a severe attack of sciatica. I prescribed a smart dose of calomel and rhubarb, to be followed by an antimonial mixture, and on the evening of the following day, injected twenty drops of Battley's sedative solution. Four hours afterwards he fell into a deep sleep, and wakened free from pain.

CASE VI.—Miss ———, æt. about fifty, very stout, consulted me some years ago, about a uterine affection, accompanied with severe lumbo-abdominal neuralgia. The pain she suffered was great, and the lameness it occasioned entirely precluded the possibility of walking. I directed attention, in the first instance, to the uterine symptoms, hoping that on their removal, the neuralgia would disappear spontaneously. In this, however, I was disappointed. Great benefit undoubtedly followed the relief of the internal disorder, and the lameness diminished perceptibly; still the pain evinced little or no disposition to abate. On the 2d June 1854, I inserted thirty drops of Battley's solution. Severe vomiting followed, and the pain was entirely diminished.

From that date, I directed my attention chiefly to the constitutional treatment, until the increased severity of the pain drew my attention to it. I injected thirty drops of Battley on the left side on the 13th December 1854.

December 14.—Pain on left side nearly gone; that on right very bad.

December 16.—Repeat the injection on the right side.

December 17.—Pain much relieved.

I saw this lady again on the 29th December The pain was much easier, but she still continued lame, and the pain was apt to increase after any exertion.

CASE VII.—A gardener, advanced in life, after exposure to cold and wet, was seized on the 18th November with severe lumbago. This had yielded by the 2d December to the usual treatment; but there remained after its disappearance symptoms of that very rare form of neuralgia described by Cotungo and subsequently by Chaussier, and denominated by Valleix crural neuralgia.

He says he has lost the power of his limbs, though this is obviously not the case.

There is a painful point in the loins, another still more marked in the groin, a third at the head of the fibula, a fourth on the dorsum of the foot.

December 12.—15 drops of Battley's solution were injected into the painful point on the loins. Next day the patient reported that he had felt no peculiar effect except that the pain was entirely gone from every point but the knee.

CASE VIII.—Mrs ——, aged about 80, has been suffering for some time from severe pain in the chest, cough, with mucous expectoration, which, together with the cough, have prevented her from sleeping for some nights. She is extremely deaf, so that it is not easy to make out her symptoms. I visited her first on the 12th December 1854. The bronchitic symptoms were then so severe that I directed attention exclusively to them. I need not detail the treatment which has no bearing on the matter on hand. On the 21st, I found the cough nearly gone, but she was still sleepless from the pain of the back, which I then for the first time examined. A painful spot was soon pointed out by the patient herself, seated near the trochanter. The integuments here were deeply seamed and scarred; the result she informed me of deep incisions made when she laboured under what she called "white swelled leg" (phlegmasia dolens I presume), 54 years before. Into this point I injected 30 drops of Battley. Next day (22d Dec.), when I visited her, she told me she had enjoyed a capital sleep; but what was that you gave me, she added, I saw the most glorious visions all night. Since then the lancinating pain has ceased, though what she describes as a dull stounding pain remains.

Case IX.—Miss ——, aged about 30. About twelve years ago, while travelling on the continent, suffered from severe influenza; while scarcely convalescent, by the breaking down of a bridge, was precipitated into a river, and had to sit some hours in a carriage with wet clothes. After this suffered from constant aching in the back, which rendered the supine position essential. Some amelioration of this took place under medical treatment. Her menstruation became scanty and painful; for this she consulted Dr Simpson, and was relieved by his treatment. Since then has had attacks of pain in back at intervals.

Last spring, pain in back became severe, extending down to knee—was recommended to take aconite, which she found to give relief. Her left side is constantly cold. When in the country, her medical attendant scarified the back and rubbed in morphia. This was done nineteen times, and she obtained some relief, but the operation was very painful.

December 24.—The pain was brought on by exertion to-day, but is not very severe. 25 drops were injected—She passed a very restless uneasy night, with much vomiting; pain in back gone.

Dec. 26.—Slight return of pain; but on the whole better. I have heard since that this young lady's health is permanently improved.

For the following cases I am indebted to Dr Thomas Wright, F.R.C.P., by whom the remedy was tried on my suggestion:—

Dr Wright's two cases treated by Dr Wood's method.

1. Mary Ann Forester applied at the New Town Dispensary on account of a painful affection of the ulnar nerve and its branches in the arm, fore-arm, and hand. During the late severe frost she had been discharged from the Infirmary cured of necrosis of the lower half of the humerus, and had afterwards been much exposed to cold, having frequently been obliged to sleep in a common stair. The limb, in consequence thereof, became severely painful, especially at night, and prevented her from either sleeping or following any occupation. She complained of occasional rigors, and there was considerable tenderness over the injured part of the

bone, but the pulse was natural and the tongue clean. Forty drops of Battley's solution were injected into the cellular tissue above the inner condyle, the limb was rolled in flannel and bandaged, she was put upon a course of alterative and aperient medicine, and ordered to visit me again in two days. Directly after the injection of the opiate the skin over, and for two or three inches round, the part became erythematous and covered with white patches of urticaria; but the severe pain in the fore-arm and hand immediately ceased. At her next visit she stated that there had been no return of the pain, but that she had suffered much from vomiting and headache, which came on directly after the injection, and lasted all the next day. She visited me only once afterwards, at which time the pain had not returned, but she had still much tenderness over the diseased bone, and occasional rigors. She was able to carry a basket of vegetables for sale.

2. Mrs A. B., a widow lady, aged 52, has been subject to periodic headache for several years. The attacks recur every fortnight or three weeks, and commence with pain in the nerves of the integument at the external angle of the right orbit and in the posterior branch of the second cervical nerve of the right side. After from four to six hours, during which time the nerves of all the right side of the scalp become severely affected, the pain either ceases altogether or passes to the other side of the head, and runs a similar course. At the commencement of a later attack, the injection of twenty drops of Battley's solution into the cellular tissue around each of the starting points of pain before mentioned was attended with entire relief for several hours, after the expiration of which the pain commenced on the other side of the head, and was attended with more than usual suffering on account of the constitutional effects caused by the opiate.

I have tried this mode of using narcotics on some other cases not so appropriate. I am quite satisfied that in those not unfrequent cases where the disease has a central, not a centripetal origin, it is quite useless, unless from the power which it may for the time exercise on the imagination.

In one case in which I tried it, by the kind permission of Dr John Brown, the puncture was, in a few minutes, surrounded by a blush of urticaria.

In considering the *modus operandi* of this new application of remedial means, I think the following propositions will guide us to a right conclusion :—

§ 1. Medicines when exhibited have usually two effects—

1*st, The local or topical*—the particular effect of the medicine on the tissue to which it is applied; 2*d, The remote effects*—being physical, chemical, or vital changes produced on parts at a distance from those to which the medicine is directly applied, or on the system at large.

§ 2. The manner in which the local effect is produced is comparatively simple, and depends on the relation of the medicine to the tissue to which it is applied. Thus, some applications simply stimulate or irritate the tissue, the effect varying from the least powerful, which merely redden, to the strongest, which produce ulceration, or even gangrene. Others, again, form compounds with the elements of the tissue, thus chemically decomposing or corroding it, while a third class, according to Dr Christison,[1] "neither corrode nor irritate, but *make a peculiar impression on the sentient extremities of the nerves, unaccompanied by any visible change of structure.*"

§ 3. With regard to the manner in which the remote effects are produced, considerable difference of opinion prevails. Magendie[2] and his supporters contended strongly that they were conveyed by absorption from the part to which they are first applied, while Messrs Morgan and Addison[3] are of opinion that the remote effects are exclusively due to sympathy, or an impression transmitted through the nerves.

§ 4. Sir Benjamin Brodie,[4] Dr Christison,[5] and others, however, unable to adopt either view exclusively, have contented themselves with admitting this double mode of operation; "a conclusion" which Messrs Morgan and Addison agree "that all fair analogy forbids."

§ 5. With the exception of Morgan and Addison, who deny the doctrine of absorption only because they strongly hold that of sympathy, and because they think it "contrary to nature's rule to adopt two ways of attaining the same end," all authors agree in admitting absorption to be the most usual channel by means of which medicinal agents are conveyed from the part to which they are directly applied, so as to effect remote organs, or the system at large.

§ 6. Since the experiments of Hering, and more recently those of Mr Blake,[6] have shown the extreme rapidity with which the round of the circulation may be accomplished, the tendency has been greater to ascribe to absorption even those very rapid, or almost instantaneous general operations of certain poisons locally applied, which were formerly regarded as the strongest arguments for the theory of nervous transmission.

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[1] Treatise on Poisons, p. 1.

[2] Magendie, Annales de Chimie et de Physique.

[3] Essay on the Operation of Poisonous Agents, &c.

[4] Phil. Trans. 1811–12.

[5] Treatise on Poisons.

[6] See this Journal, vols. liii. and lvi.

§ 7. Of the great effect of absorption we will be still more convinced, if we call to mind the rapid disappearance of the agent from the part to which it was applied,[1] coupled with its speedy detection in parts at a distance,[2] and the no less speedy communication of its qualities to the animal solids and fluids.[3] The arrest of the action of the poison by arrest of the circulation from the part,[4] and the failure of all attempts to arrest the production of remote effects by intercepting the nervous communication.[5]

§ 8. The chief agents by which absorption is effected are the veins, though the lacteals and absorbents take up certain agents, but their operation is both limited and slow.

§ 9. Different tissues vary in their absorbent power. Thus, according to the very interesting experiments of M. Vernière,[6] the mucous membrane of the intestinal canal absorbs less rapidly than the serous membranes, and they, in their turn, are less powerful channels of absorption than a vein or an open wound.

§ 10. Difference of tissue is therefore found to modify, to a great extent, the action of remedies. Thus, the stomach and intestines, which are the tissues to which medicines are generally applied, possess a considerable power of absorption, as indeed their office would lead us to suppose; nevertheless we find, from the experiments of Christison and Coindet,[7] "that when oxalic acid is introduced under the same collateral circumstances into the stomach of one dog and the peritoneum of another, the dose may be so apportioned that the same dose which does not prove fatal to the former kills the latter in fourteen minutes."

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[1] Of four ounces of solution of oxalic acid, injected by Drs Christison and Coindet into the peritoneum of a cat, and which proved fatal in fourteen minutes, though none escaped by the wound, scarcely a thirty-second part was found after death.—(See this Journal, vol. xix. 335.)

[2] As in the urine, see the very full experiments of Wöhler and Stehberger (Zeitschrift für Physiologie, Bd. für 1824–5.) Or, as in a case quoted from Fricke by Pereira (vol. i., p. 106), where iodine was detected in the tears by the formation of iodide of mercury, when calomel was applied to the eye of a patient who had been taking iodide of potassium. In the blood and chyle, as by the experiments of Tiedemann and Gmelin.

[3] Almost all the minerals, and many other substances which are given medicinally, have been detected in the bones, brain, skin, and liver (Pereira, op. cit., p. 104.)

[4] See the experiments of Ségalas, Emmert, and Blake (Muller's Physiology, and op. cit.)

[5] See the experiments of Magendie and Delille (Physiology by Milligan, p. 284.)

[6] Journal des Progres, 1827.

[7] See this Journal, vol. xix., p. 330, and Christison on Poisons, p. 29.

§ 11. The few experiments which we had, until very recent times, regarding the power of the pulmonary membrane to absorb poisons, tended to cause it to be regarded as a channel of extreme power. The rapidity of the fatal results which follow gaseous poisoning may be instanced, as also the experiment of M. Ségalas, who found that half a grain of solution of extract of nux vomica injected into the windpipe proved fatal, while two grains might be injected into the stomach, peritoneum, or chest, without any fatal effect.

§ 12. The experiments of Professor Simpson on the inhalation of chloroform, oil of juniper, ergotine, and other medicinal agents, also show the value of this channel for the introduction of certain medicaments into the system.

§ 13. The skin, which has at various times been employed as a medium for the introduction of medicines into the system, would appear to possess no very active power of absorption, at least unless it be denuded of its cuticle. "Accordingly," says Dr Christison,[1] "many active poisons are quite inert when applied to the unbroken skin, or even to the skin deprived of the cuticle. Hydrocyanic acid, perhaps the most subtle of all poisons, was found by Coullon to have no effect when dropped on the skin of a dog." On the other hand, Dr Madden, in his work on Cutaneous Absorption,[2] has shown, from carefully conducted experiments, the power which the healthy skin possesses of absorbing from a gaseous and from an aqueous medium, and has collected from various authors proofs of its power to absorb medicinal substances.

§ 14. Both solids and fluids have been thus absorbed by the skin. Kellie found salivation follow the use of a mercurial plaster. Arsenic employed to destroy lice has been known to produce violent inflammation. Salivation has been produced by the absorption of a solution of corrosive sublimate. Dr Madden, after immersing his hands in a solution of hydriodate of potass, detected iodine in his urine, and he also succeeded in purging himself, by applying to his skin infusions of rhubarb, jalap, and gamboge.

§ 15. There is no more rapid way of securing the action of poisons than by introducing them into a divided vein, or into several divided veins, by means of an open wound.

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[1] On Poisons, p. 28.

[2] An Experimental Enquiry into the Physiology of Cutaneous Absorption, by W. H. Madden, M.D. Edin., 1838.

"Some," says Dr Christison, "which act very slowly through the stomach cause instant death when injected into a vein."

§ 16. With regard to the cellular tissue, Dr Christison states, "that it is a ready medium for introducing poisons into the blood, especially if an artificial cavity be made where the tissue is loose, but that its power as a medium of absorption has not been, and cannot easily be ascertained. On the one hand, it is difficult to apply poisons to it without also applying them to the mouths of divided vessels, and, on the other hand, it is difficult to make a set of experiments for comparison with others on the stomach, pleura or peritoneum, as the cellular tissue does not form an expanded cavity, and, consequently, the extent of surface to which a poison is applied cannot be made the same in each experiment of a series."[1]

§ 17. The experiments which approach the most nearly to direct injection of the cellular tissue are those detailed by Sir Benjamin Brodie in the Philosophical Transactions for 1811–12, in which various poisons were introduced into wounds, and were found to produce very speedy results; but in all these cases the great division of parts exposed so many blood-vessels that it is not easy to say how much of the effect was due to the cellular tissue, and how much to the action of the divided vessels.

§ 18. I am at present engaged in some experiments on this subject, in which, by means of an improved apparatus, various substances have been introduced into the cellular tissue with comparatively little injury to the adjacent vessels, and, as far as these have gone, they would lead to our ascribing great absorbent power to the cellular tissue.

§ 19. The result of what has been stated proves satisfactorily,—

1st, That medicines are more rapidly absorbed by some tissues than by others.

2d, That the stomach is by no means the most rapid way of introducing medicines into the system.

3d, That the cellular tissue has a great power of absorption.

§ 20. It has been further shown by Dr Christison, that the whole amount of difference is not to be explained by the rapidity with which absorption goes on, but is to be ascribed in part to the poison being more liable to decomposition in one tissue than in another. Thus, many remedies are much changed in the stomach, where the powers of assimilation are very strong, and the action of absorption slow.

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[1] On Poisons, page 30.

§ 21. We are thus conducted to this point of the enquiry. Have we no means of introducing medicinal agents very rapidly into the body, in a situation where they will not be readily decomposed, and where, in certain cases, we can secure at once their local and their remote effects.

§ 22. The value of such a method of procedure, in many diseases, will be apparent. We shall instance but one—neuralgia.

§ 23. In neuralgia we have usually a general and a local affection, a morbid state of the system, arising from many causes, displaying various symptoms, requiring varied treatment, and existing in states of the body the most opposite; a local affection, occurring in paroxysms of violent pain, either regular or irregular, plunging like electric shocks along the course of the affected nerve, ceasing, either to be suspended for a time, or immediately to recur with still more unbearable violence.

§ 24. An affection presenting characters such as have been described, would appear to demand at once a local and a general treatment;—a local treatment directed to, and intended to mitigate the fearful anguish, under which the patient is well-nigh driven to despair; a general treatment intended to correct the "*habitus neuralgicus*" on which it depends, and having reference to the causes from which it has arisen, the state of the system in which it exists, and the diseases with which it may be associated.

§ 25. There are other circumstances in the history of neuralgia which seem to point at, and to give peculiar facilities to local treatment. It is admitted on all hands that the *superficial* nerves are of all others the most liable to the disease. It has further been shown by M. Valleix[1] that some points in the course of the nerve are more liable to be affected than others, and that these points are frequently the very ones where the nerve is most superficial.

§ 26. Further, these points can almost always be detected in the course of the disease from their extreme tenderness on pressure. Even in the intervals between the paroxysms, very slight pressure on these points is sufficient in many cases to excite severe suffering, although, in some exceptional cases, firm pressure may be applied without exciting any complaint.

§ 27. The plan of local treatment which M. Valleix proposed was the application of a succession of small blisters over the points in the course of the nerves which are painful on pressure, and in all his cases it seemed to alleviate the symptoms.

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[1] Traité des Néuralgies, ou Affections Dolowreuses des Nerfs. Par F. L. S. Valleix. Paris, 1841.

§ 28. The plan of blistering is not new, but for the application of it to the tender points we are indebted to M. Valleix, and that author has effected an immense improvement in practice by showing where our local treatment, whatever that may be, ought to be applied.

§ 29. Our own experience has not confirmed the value of simple blisters, and we prefer following them up by the endermic application of morphia.

§ 30. Two strong objections, however, apply to blistering, or the endermic application of narcotics in this disease—

1st, The painful nature of the remedy.

2d, The mark which it often leaves, which is very objectionable when the disease is seated in the nerves of the face.

§ 31. Various methods of applying narcotic or other remedies more directly to the seat of the disease have been introduced. Thus we have—

1st, The *enepidermic method*, in which the agent is simply applied to the surface of the skin.

2d, The *Iatraleptic*, in which the absorbents are stimulated by friction to take up the agents which are presented to them in solution or in a minute state of division.

3d, The *endermic* proposed by MM. Lembert and Lesieur, in which the obstacle which the epidermis offers to the entrance of the remedy is overcome by previously removing it.

4th, *Inoculation*, which, largely practised for the introduction of small-pox and cow-pox into the system, has been proposed by M. Lafargue St Emilion, to be extended so as to secure the application of remedies. This method was brought before the Westminster Medical Society in February 1837, by Dr Bureaud, but from the account given in the 'Lancet,'[1] he does not seem to have been very successful, as only a slight local effect was produced. A report was made by M. Martin Solon for the Academy of Medicine,[2] on this method of inoculation by morphia, proposed by Dr Lafargue, which report comes to the somewhat damaging conclusion that the effect produced was very much the same, whatever agent was inoculated even when the experiments were made with agents as dissimilar as belladonna, strychnine, the gastric juice, chyme.

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[1] Lancet, 1837, p. 826.

[2] Bullet. de l'Acad. Roy. de Med. 1836, vii., Nos. 1 and 7.

§ 32. From all this it is plain that we are still in want of a method of directly applying sedatives to the affected part, and that could such a method be suggested its value would be enhanced, could this be done almost without pain, and in a manner calculated, at the same time, powerfully and rapidly, to affect the general system.

§ 33. Of the value of such an application locally to the nerve affected, no one can be in doubt who calls to mind the result of the experiments instituted by Müller, which clearly shows that, to quote his own words, "narcotic poisons," when applied locally to nerves, have only a local effect. I held the nerve of a frog's leg which was separated from the body in a watery solution of opium, for a short time, and that portion of the nerve lost its irritability, but below the part that the poison had touched the nerve still retained this function; *opium, therefore, produces* a change in the nervous matter itself, but the influence is local.[1]

§ 34. Again, every one who has seen much of neuralgia is aware that, on the one hand, the pain, acute and agonising as it is generally, subsides spontaneously after some time; that on the other, opiates administered through the ordinary channels are usually some hours in taking effect, so that, if this class of remedies are to be of use at all, it must be an immense advantage to secure—

1st, A local effect, applied *directly* to the affected nerve.

2d, A remote effect, ensuing almost *instantaneously* on the application of the remedial agent.

§ 35. Several of the cases which I have detailed show with what rapidity narcotics take effect when introduced in this way; and in a case in which I tried it in the Royal Infirmary, through the kindness of my friend Dr W. T. Gairdner, the man, who was not at all aware of what was doing, told us that he felt as if he was drunk within a very few minutes after the introduction of the narcotic.

§ 36. These are the advantages which this new method of treatment seem to offer, and on which we venture to recommend it for trial; and I think we may safely arrive at the following important conclusions regarding it from the cases which I have submitted—

1st, That narcotics injected into the neighbourhead of the painful point of a nerve affected with neuralgia, will diminish the sensibility of that nerve, and in proportion diminish or remove pain.

2d, That the effect of narcotics so applied are not confined

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[1] "Physiology," by Baly, vol. i., p. 246.

From a Photograph in 1854

Schenck & McFarlane Lithrs, Edinburgh.

Ever yours sincerely
R. J. Mackenzie

to their local action, but that they reach the brain through the venous circulation, and there produce their remote effects.

3d, That in all probability what is true in regard to narcotics would be found to be equally true in regard to other classes of remedies.

4th, That the small syringe affords a safe, easy, and almost painless method of exhibition.

5th, That, destitute as we are of any precise experiments as to the applicability of cellular tissue as a medium for the reception of medicinal agents, the experiments made with the syringe show that it seems to offer an excellent surface for the absorbent action of the venous system.

6th, That the method now detailed seems as extensively applicable as any of the methods of applying remedies to the skin, whether

Enepidermic,
Iatraleptic,
Endermic, or by
Inoculation.

Art. V.—*Brief Memorial of the Life and Writings of the late Richard James Mackenzie, M.D., F.R.C.S.*

Though departing from our usual practice, we should be doing violence to our own feelings were we not to offer a tribute of respect to the memory of one whose character, professional and private, the incidents of whose life and death, and whose intimate connection with ourselves in conducting this Journal, alike demand it.

Richard James Mackenzie, the fourth son of the late Richard Mackenzie, Esquire of Dolphinton, and deputy-keeper of Her Majesty's Signet, was born in Edinburgh on the 31st of March 1821. His early education was, in the first instance, conducted by Mr White, long eminent as a teacher of English in this city, and thereafter in the New Academy, which he entered in 1829, and where he continued uninterruptedly during the full curriculum of seven years. After leaving the Academy, where he distinguished himself, and was successful in obtaining several prizes, he received instruction from a private tutor, along with his early friend and future fellow-student in medicine, Walter Dickson, till entering upon his professional studies in the autumn of 1837. With Mackenzie the choice of medicine as a profession was no sudden determination; on the contrary, from his boyhood he had resolved to be a "doctor," and, without the slightest swerving, he kept that resolution. In 1838 he was bound apprentice to Dr Adam Hunter, Fellow of the Royal College of Surgeons, a plan of education then becoming much less common than formerly, and now, from obvious causes, totally abandoned. Of his connection with Dr Hunter, Mackenzie always spoke in warm terms, and frequently referred with evident pleasure to the period of his apprenticeship. In January 1839 he became a member of the Royal Medical Society, and during the session 1840–41, along with Professor Day of St Andrews, Professor Thomas Anderson of Glasgow, and Dr Hugh Cleghorn of the Indian Army, he was elected a President of the Society. It is difficult to say at what precise period of his studies his preference for that department of the profession in which he was destined afterwards so signally to excel, was aroused. If the determination to devote his special attention to surgery was formed antecedent to his entering the Royal Infirmary in 1840 as resident clerk under Professor Syme, we can easily understand how, when in that position, the determination was fostered. The two years passed in the Infirmary was one of the happiest periods of Mackenzie's life, and he was always accustomed to refer to it as such. With almost unlimited opportunity of observation in his now favourite walk, attached to a master for whom he entertained then, as throughout life, the highest respect, and associated in the Hospital with those[1] he liked and esteemed, it is not surprising that the days passed pleasantly by. On the 1st of August 1842, having previously obtained the diploma of surgeon, he graduated in medicine, his inaugural dissertation being entitled "Practical Observations on Injuries of the Head." After a short Highland tour and a few weeks spent with his family at Dolphinton, his usual residence at a holiday time, where, engaged in field-sports, in which he took great delight, and in some particularly excelled, Mackenzie, after visiting the hospitals in London, proceeded to complete his professional education in the chief schools on the continent. The winter session of 1842–3 was spent in Paris, where, judging from his full, and in many instances most elaborate, notes of cases in the hospitals, he seems to have been most attracted by the practice of M.M. Velpeau and Malgaigne and the late M. Roux. Surgery, however, even in the French capital, did not monopolise his whole attention: his note-books are occupied with the description of many very interesting purely medical cases, a fact which, taken in connection with his habitual practice and his known opinion, shows that Mackenzie, though desirous to devote himself to the study of surgery, believed that a thorough knowledge of medicine was no less essential to the future surgeon.

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[1] Dr Halliday Douglas was among the number.

Leaving Paris early in the summer of 1843 Mackenzie returned for a few weeks to Edinburgh, and thence proceeded to Hamburg, where he continued to reside for some months, his main object being the acquirement of the German language, with a view to prosecuting his professional studies during the following winter at Vienna and Berlin. He then made a short tour of Switzerland and the north of Italy, and thereafter, in company with a friend who had joined him from Scotland, he passed through the Austrian Tyrol to Munich, and thence, by Salzburg, through the beautiful Salzkammergut to Lintz and Vienna. Of the latter part of this expedition Mackenzie kept a regular journal, in which the beauties of the country, besides being praised, are most accurately described, the only unpleasant recollection being caused by his detention at Ischl on account of the serious illness of his friend and fellow-traveller, Mr C. At Vienna Mackenzie remained for some time; but the greater portion of the winter of 1843–4 was spent at Berlin. From this city, his residence in which he had greatly enjoyed, he passed almost directly to England, and, after remaining a further time in London, reached his home in Edinburgh, after an absence of fully two years, in the autumn of 1844. Very shortly after his return we remember to have heard him read a paper to the Royal Medical Society, in which the chief points of agreement and variance in the practice of British and French surgery were pointed out and examined, forcing on our own, as on the minds of all who were present, the conviction that the same earnest thirst after knowledge which had distinguished him when a student in Edinburgh had animated him when abroad. He now became a Fellow of the Royal College of Surgeons, a medical officer of the New Town Dispensary, a member of the Medico-Chirurgical Society, and established himself in practice. In 1848 he was appointed an Assistant-Surgeon in the Royal Infirmary. In the previous year he married. His union, of the happiest character, was destined to be a short one. Within a year after giving birth to a daughter his wife was removed by the hand of death. This was a severe blow to Mackenzie, and for a time he appeared to be completely overwhelmed. Those, however, who knew him well, and are best entitled to speak on such a subject, know that in him that fiery trial was effectual, as no doubt it was intended to be, in "yielding the peaceable fruit of righteousness." It was after this sad event that Mackenzie's more prominent career of professional usefulness commenced. In the autumn of 1849 he succeeded Dr Argyle Robertson as lecturer on systematic surgery in the Extra-Academical School, and in the following year became one of the acting surgeons of the Royal Infirmary. During this period, too, his private practice was rapidly increasing, and, along with it, his eminence as a skilful and successful operator. Few of his standing have ever been so universally respected by their professional brethren by reason of his acquirements, while his great amiability of character ensured him friends wherever he went, and closely attached many to him. It was when his reputation was thus steadily advancing that in last spring he conceived the idea of proceeding to join our army in the East. Before, however, finally determining upon the step he afterwards took, Mackenzie sought the advice of many of his professional and private friends; and if at the first it did occur to some, for whose opinion he entertained the highest respect, that the project was attended with such risks as to lead them to dissuade him from it, all such will be ready to acknowledge that, after hearing from himself his reasons for going, and what he hoped to accomplish when there, their objections were in great measure removed, and that, along with the many who from the first applauded his determination, they were ready to wish him "God speed." Here, modestly expressed in his own words from a letter to his eldest brother, is the reason he assigned for going:—"The object of my expedition to Turkey is to gain professional information, and, in doing this, to render all the assistance in my power to the troops; and from the fact of my having had the privilege of being acting surgeon to a large public hospital for a good many years, I ought to be able to render some efficient assistance." On Mackenzie's wish to proceed to Turkey being made known through a friend to the Earl of Aberdeen, the then Premier, he was in the kindest manner furnished by his Lordship with a letter of introduction and recommendation to Lord Raglan, containing a request that on his arrival he should be placed in relation with the medical department of the army. Being provided, through the kindness of friends, with numerous other letters of introduction to persons high in military and civil rank in the East, Mackenzie, in excellent health and high spirits, bade a final adieu to his friends and to Edinburgh on the 13th of May. In London he remained for a few days enjoying the encouraging counsel of his always kind friends, Professor Fergusson and Dr Robert Taylor, and, among other things, in receiving hints for his expedition from one well qualified to give such advice—Mr Guthrie.

Leaving London on the 17th, he spent two happy days with his friends, Mr and Mrs W. Freeman, at Pylewell, in Hampshire, and on the 20th, in company with Mr Tufnell of Dublin, as previously arranged, he left Southampton in the "Colombo" for Malta. In the passage from Malta in the "Carnac," he had on board his friend and pupil, John Nasmyth, who had been travelling in Egypt on account of his health, and was anxious to see Constantinople before returning to England. Arrived in Turkey on the 6th of June, he thus writes to his brother George on the following day:—"We had a beautiful passage from Malta of four and a half days, stopping at Syra, one of the islands of the Archipelago, for six hours of Sunday afternoon. The sea the whole way from Malta was like a mill-pond, and the weather clearer and hotter than anything I ever felt in my life before. The scenery, after coming in sight of the Morea, was very fine, and still more so after we got into the Dardanelles, while the beauty and splendour of the approach to Constantinople yesterday morning was what I shall not attempt to describe . . . . To-day I have spent most of the time at Scutari. The encampment is a beautiful sight, including, besides a large force in a splendid barrack, the Guards, 55th, 41st, 30th, 47th, 95th, and 49th, and the Highland Brigade (93d, 79th), (and 42d arrived to-day) in tents. I went straight to Dr Linton, who expected me, then met T. Scot, Adam Maitland, and a host of old friends, too many to enumerate. I then went to Lord Raglan, presented my letter, and had a quarter of an hour's interview with him. He is to give us every assistance. Colonel Steele, his aide-de-camp, is to arrange for our reception in the Turkish camp, rations, &c. . . . . . . I then went and spent an hour with my 79th friends. My reception with them, and with all, was most agreeable . . . . . T. Scot is in great force, and does not know how to be sufficiently hospitable. He, Adam Maitland, and W. Scott, besides one or two others, talked of me as their property for the campaign, and I found T. Scot had actually arranged with the Colonel that I should be with them when they advance."

No wonder Mackenzie liked the Cameron Highlanders when this was the reception he met with in their camp, a reception which shows the peculiar suitableness there was in his future time being spent chiefly with them. Remaining, however, with the 79th, he goes on to say in the same letter to which we are already so indebted:—"Won't suit me now, for the British force is evidently not going to be engaged for some time—I hope before I leave this—and the Turks are now our only chance." Shortly after this, therefore, in company with Mr Tufnell, Mackenzie left for Varna, and thence the following day to Shumla, where he remained ten days. "My journey there," he says in a letter to a medical friend, "was of the most fatiguing kind—ten hours each day in the saddle under a burning sun, and very little either to eat or drink. My stay at Shumla was very interesting professionally, but it was the pursuit of knowledge under no small difficulties and discomforts. No servants, and a miserable hovel to live in, the care of two horses each, and nights spent among myriads of fleas, &c. In spite of these annoyances I now rather regret that I did not remain longer, but I came back here (Aladin), to join our own troops, in the expectation that they would immediately march to the relief of Silistria, where I then believed I should have seen a general engagement within a very short time. Now that matters are so much changed I half regret I did not accept Omar Pasha's offer of service with him, which was made to me at Shumla when I had the honour of an interview, and of drinking coffee in an arbour, with the great general. As you know, however, I have perfect faith in everything being ordered for the best." Returned to Aladin on the 8th of July, Mackenzie now remained in the camp of the Highland Brigade, induced to do so by the constant rumours of an immediate advance of the allied forces. He was about this time on the sick list for a few days from exposure to the sun. "My brow and face got red and swollen," he says in a letter to Dr Warburton Begbie, dated July 16th, "in fact I had a slight attack of erysipelas, which, however, has had no more serious effect than disfiguring me for the time being. Our friend Linton did not recognise me this morning; and then, after a good fit of laughing, said he wished some of our friends in Edinburgh could see me." Recovered from this indisposition, in company with six officers of the Highland Brigade, he made an excursion to Silistria, from before which the Russians had then retired. On the journey, which was a very fatiguing one, his ankle was sprained, and during the whole time he was in Silistria he was unable to put his foot to the ground. He comforted himself in the disappointment of not being able to enter the hospitals there, by the reflection that the surgery of Silistria would not differ in any great degree from that of Shumla.

On the 30th of July he was again in the camp of the 79th at Gervehler, a table land on the top of a steep hill some twelve miles from Varna, and nearly a thousand feet above the lake of Devna.

Lord Raglan had by this time sent an official permission to Mackenzie to attach himself to the 79th regiment as long as he wished to remain, and to draw rations and forage for himself, servant, and horses; while he was occupying himself in the hospitals for some hours daily in the treatment of cases of pneumonia, slight dysentery, and remittent fevers, the only diseases then prevalent, Alas, in the course of a very few weeks a great change had occurred. On the 1st August Mackenzie writes from Gervehler,—"Every one is in low spirits to-day, and somehow I can't help feeling depressed. We have unmistakeable evidence of the troops becoming unhealthy, and every one has the uncomfortable feeling of the approach of some epidemic. Already we have had a considerable number of cases of cholera, nearly all of which proved fatal at once . . . . T. Scot has fifty-four of his regiment in hospital . . . . . The climate is now more like our own than I have yet seen in Turkey, and it is odd that this is the weather in which we are threatened by an epidemic." In the same letter occur passages which are now, in one point of view, very painful to read, but which we shall not withhold, exhibiting, as they do, in a singular manner, the noble disinterestedness which actuated him. "Please God to spare me I shall be at home this day two months, and I almost hope sooner. Yet I do not like to go as long as there is any near prospect of the troops being engaged, nor do I like the idea of shunning an epidemic in which I might be of use." The epidemic did come;—it was assuredly not what Mackenzie had left his home and gone to Turkey to see, but nobly he did his duty. "The cholera at Gervehler," he writes, "was the most virulent specimen of the disease I have ever seen. Many were struck down at morning parade, and died in three, four, or five hours, I need scarcely say that any treatment in such cases was utterly vain. Taking the whole of the cases, however, our mortality was not great, something about 40 to 45 per cent." In another letter, dated August 3d, he says,—"Several officers have died of cholera, and poor Colonel M. among the number. His death has made a great sensation, especially in our regiment, of which he was long colonel, and an especial favourite . . . . . We have thirty men in the hospital, chiefly from fever. Our colonel is in a very critical state with remittent fever. Our major, too, has it, and looks bad." On the 12th of August he wrote to his brother George,—"I am very well. I think it as well to commence with this, as I feel sure that the news of the sickly state of the camp conveyed by this and the last mail will have struck a panic into people at home, and made one's friends feel very anxious . . . . . . We have lost our colonel and young T. of the 93d. I was in constant attendance upon both, along with their respective surgeons. . . . . On Friday I went to Varna in charge of Colonel F.; took him on board the 'Emu,' and remained an hour with him. I felt anxious about him. He had remittent fever, but we trusted to sea air renovating his health." It did not do so, and the news of his death received from Gallipoli struck heavily on the hearts of the whole camp, and on no heart more heavily than Mackenzie's. These extracts from his own letters show how, during the sad period of sickness at Gervehler, Mackenzie was employed: let the following extracts from the letters of different officers show how his attentions were valued. Captain K., of the Guards, writes,—"I have renewed my acquaintance with Dr Mackenzie; he has been most kind and attentive to several officers; in fact, making himself generally useful. He is a great addition to the medical staff and to society. He has gained golden opinions from every one." An officer in the Scots Greys, speaking of Mackenzie's attention to the private soldiers, says "he was most kind to any of the men who fell behind during the march, always remaining with them for some time, and then running on and catching up the regiment." The following beautiful testimony to Mackenzie was written on the 23d August by Captain K. M., of the 79th regiment:—"At Gervehler, I am sorry to say, we left Lieutenant H. dying; that good Samaritan, Dr Mackenzie, stayed behind and saw the last of our poor friend. No man has the good wishes of the regiment so thoroughly as Mackenzie; a kinder and better man never was, and I hope he may some day receive his reward. No matter the rank of the patient, every one receives the same attention."

At length the period arrived when an opportunity was to be afforded him of witnessing a general engagement, and of extending his valuable services to the wounded. This was what had induced him to leave England, and the prospect seemed to cause him to forget the annoyance of the long and somewhat monotonous delay which had occurred. His letters are for the future written in better spirits. On the 28th he wrote to his eldest brother from Galata, where the 79th was then encamped:—"The rumours which have been so prevalent for some time of our army being about to do something have now apparently been changed to a certainty . . . . . We have little doubt that we are going direct to Sebastopol. I cannot doubt you and all at home approve of my having remained, and of my going where there is so much prospect of my being useful, and of my seeing so much . . . . . Now, though no doubt I, with all the other surgeons, will be well cared for, I have no doubt that, more or less, we shall be under fire, and consequently I shall be to some extent exposed, as far as risk of life goes. I am quite prepared for this, though I cannot say I am troubled with any *presentiment* or similar absurdities. I shall go determined, like all the others, to do my duty, but will expose myself to nothing that I can avoid, unless it is my very clear duty to do so." Concerning his own health, he wrote in the same letter,—" I am thankful to say I am very well now, though, to tell the truth, since the middle of July (except on my Silistria trip) I have been very shaky. During the time of cholera I had an attack of diarrhœa, which weakened me very much for the time, and once I had, like several others, a slight attack of dysentery, which made me think seriously of leaving, and made T. Scot advise me to be off. On the top of this I got a curious attack of neuralgia in my foot, which now and then made me dead lame for hours at a time. From the day I left that vile place, Gervehler, I have been as well as I ever was in my life, and am now in perfect health ; and in the few days I have been here I think I have picked up all the flesh I lost at Aladin and Gervehler. We are in a very healthy position, and have glorious sea-bathing every day. I have lots of friends all round me, and am on the best of terms with what I may now call my own regiment. I believe it is likely I am to be temporarily incorporated with the 79th now, or at least to hold military rank (*pro temp.*) in the Highland Brigade. It is a matter of no moment to me, but my friends here think it better I should appear on the roll of the regiment. I shall, at all events, appear before Sebastopol in tartan breeks and a shell jacket, to be like my neighbours." The very day on which the letter we have quoted from was written a sudden order arrived for the regiment to march at daylight to the shore, which it did, and immediately embarked on board the transport ship "Dunbar." On the 3d of September he wrote from the " Dunbar " as follows :— " The work of embarking troops has been going on at a prodigious rate the whole week, and now all our troops, and nearly all the French, are said to be on board ship. We are very comfortable . . . . . What a sight! 30,000 French, 22,000 English, and 20,000 Turks, and the whole allied fleet. It is, I suppose, the most formidable expedition ever undertaken of the kind, and I am very glad to have had the good fortune to see it. There is something very awful in thinking of the struggle which must so shortly ensue ; but here, and on board all the ships I have been in, every one is in the highest spirits, and I cannot help participating in this feeling myself. ' Sufficient unto the day' is, I think, peculiarly the soldier's watchword, and it is fortunate it is so." Landed in the Crimea, after two days' bivouack in the open air, he thus wrote,—"Here we have been in the enemy's territory for two days . . . . roughing it in good earnest, but, on the whole, very jolly—living on biscuit and salt pork, cocoa, tea, and sugar . . . . . . We have been wrapped in blankets—clothes on—on the hill-side . . . . My name has appeared in general orders, as serving as full surgeon in the army of occupation, and with full pay and field allowances. I am now, of course, in uniform, and have a regimental servant . . . . . The landing, as the papers of course have told you, has been most deliberately and successfully managed . . . . Time is up. I am very well. Every one is in high spirits." To Dr Douglas Maclagan he wrote,—"The troops are in capital condition, in spite of all our sickness and all that may be said to the contrary, and they are all eager for the attack. I feel the same myself, and am only sadly annoyed at having been delayed so long. Our sea voyage has done much to revive the men, who a month ago looked very shaky. Cholera, however, has not ceased. The 42d have lost nine men, and the 33d fourteen men, from cholera since we sailed. We have had two or three cholera cases, which, however, proved quite manageable." The allied armies advanced, and on the 20th of September, encountering the enemy, fought the battle of the Alma, and achieved a brilliant and now illustrious victory. Fortunately others have recorded his heroic and humane deeds on that day and field, for the part Mackenzie took can only be faintly gathered from the letter (the last he ever penned) written to his brother Kenneth on the following day. "The papers will have told you of our severe action of yesterday. It was truly a terrible action as long as it lasted (two hours and ten minutes by my watch). It is a glorious victory, considering the strength of the Russian position. When I looked at it, as the action commenced, it seemed to me impossible that they could be dislodged. It was a glorious and most exciting scene, though very sad, to see our columns march steadily forward under the mowing fire, and carry their redoubts and breastworks, one by one, at the point of the bayonet, and with scarcely a check. They had far larger guns than we expected, and T. Scot and I, without intending, got within their range. . . . On the enemy retiring we followed in the rear of our brigade, and as far as I can learn we were the first on the field to render assistance. What a sight! Reading of it gives no idea of the field of a murderous battle such as our passage of the Alma yesterday. There was the excitement of victory, however, to counteract the effects of the dismal scene; but this morning it floored me when I went over the field, the ground strewed with the ghastly dead and wounded. I could not stand it, and went off to the *rendezvous* where the wounded were brought. There I have been from eight till six P.M., and I am exhausted with sheer hard work. I cannot tell you what number of limbs I have removed to-day. I have operated from morning till night. . . . . . I have *lost* and *made* many friends to-day and yesterday. The first officer I found on the field was poor young A., quite dead, shot through the heart. . . . . You will see how slightly the 79th have suffered. T. Scot and I had all our wounded attended to, and were up with the regiment on the top of the height an hour after the action was over. We were shaking hands with all our friends, when, to my no small surprise and gratification, as you may believe, a voice shouted out from the column, as they stood in the ranks—'*Three cheers for Mr Mackenzie*,' and though I say it, who shouldn't, I never heard three better cheers. You will *laugh*, my dear fellow, when you read this, but I can tell you I could scarcely refrain from doing t'other thing. All I could do was to wave my Glengarry in thanks." Mackenzie's coolness under fire is thus alluded to by the quarter-master of the 79th, in a letter published in the "Edinburgh Courant." During the height of the action I was in conversation with him, when a round shot struck the ground, and rebounding over our regiment, flew over our heads and killed an artillery horse a few yards in our rear. Mackenzie quietly remarked, "That is a narrow escape." The same officer says, "Mackenzie performed twenty-seven capital operations with his own hand, two of them amputations at the hip joint." The dead being buried, and the wounded attended to, the army marched after the battle of the Alma; Mackenzie still with his Highland friends, who, on the 24th September, were encamped at Bornoo, on the top of the high ground on the south side of the river "Katcha," and about three miles from Sebastopol, in the direct line. On the afternoon of that day, we learn by a letter from an officer of the Rifles, that "Mackenzie appeared in good health and spirits." No doubt, however, he was much fatigued. After the battle he said so himself, and the heavy march of the 24th and previous day must have added to it. By some he is said to have incautiously eaten of grapes, but however this may be, he seems to have had no premonitory symptoms of illness, at least of any consequence. The letter of his devoted friend Dr Scot of the 79th, to which we shall now turn, expressly states that his seizure was a sudden one:—"Your dear brother Richard," writes Dr Scot to Mackenzie's eldest brother, "was suddenly seized with Asiatic cholera on the night of the 24th instant, and died on the following morning at nine o'clock. You may rest assured that every thing I could do for him was done, as I at once called in the medical men in whom I knew your poor brother had the most confidence. He was taken ill about eleven o'clock P.M., without any premonitory symptoms; the attack was a most virulent one, and he sunk rapidly. He continued quite conscious till within half an hour of his death. He desired me to tell you and all his friends that he was quite composed and prepared for death, if God so willed it. . . . . The Church of England clergyman was sent for, and prayed with him and for him in the most simple and beautiful prayer; and though before this his voice had almost failed, he said, 'Amen' at the conclusion, with a firmness and distinctness which surprised me. . . . . We buried his remains close to the spot where he died." It is mentioned in an officer's letter that, on this sad night, when owing to the alarm of the enemy approaching, orders had been given to extinguish all the camp fires, Sir Colin Campbell would not allow the fire near which Mackenzie lay to be so treated, preferring the risk of a surprise to removing that little comfort from one who had proved himself so true a comrade. Thus Mackenzie literally met a soldier's grave, and within an hour after his burial, of the mighty host that, during the previous night, had encamped on the heights of Bornoo, there remained but that one dead surgeon—

> "We carved not a line, and we raised not a stone,
> But we left him alone with his glory."

"Many a true tear of sorrow was shed over his grave," wrote one of Mackenzie's most attached friends—himself a real soldier and most amiable man—who, too, within ten days fell a victim to the same disease. Had it been our object to write a memoir of our departed friend instead of a brief memorial, we might have spoken of the manner in which his loss was felt, both in the army he accompanied and in this country, particularly in this city, when the sad news reached it. We might tell how his loss is now felt; but the united testimony of the public prints renders that a work of supererogation, and we prefer to conclude in the words of another, who was present with Mackenzie to the last—"Deeply," writes Captain M., "we regretted his loss, and that for many reasons. He had submitted to all privations like ourselves with the greatest cheerfulness, always the first to assist the sick, to join in any duty, and any pleasure—anything social was sure to be enlivened by his presence. He was the best-hearted, least selfish, man I ever met—as a Christian, as near perfection as possible."

Dr Mackenzie's Contributions to Surgery.

In the following notice of Dr Mackenzie's surgical writings, we have kept in view the twofold object of drawing attention to the important cases and facts he has recorded, together with his views or opinions, and of assigning his merit as a contributor to the advancement of surgery.

I. *Ligature of the Subclavian Artery.*

This his first paper is entitled "Ligature of the subclavian artery, in a case of hemorrhage from the axillary artery," (*Northern Journal of Medicine*, March 1846). The patient had accidentally received a severe burn with a red hot poker deep in the axilla. A week afterwards, the first of several copious bleedings took place, and, a fortnight after the accident, Dr Mackenzie tied the subclavian artery. The ligature separated on the 20th day, and the result was altogether successful. As he remarks, "the practice to be pursued in similar circumstances would still be a matter of considerable doubt in the minds of most surgeons." The impossibility of securing the axillary artery above and below the bleeding point is evident from the details given; and the other alternative, amputation at the shoulder joint, was kept in view as a last resort, should the ligature of the subclavian artery have failed to arrest the bleeding, or have produced gangrene of the limb. The patient was dependent on the use of his right arm, and was able to resume his duties as a clerk on the twenty-second day after the operation. There can be no doubt as to the propriety of the course pursued by Dr Mackenzie in this case. The subclavian artery was easily reached, and only about a sixth of an inch of it denuded. The ligature was easily carried round with a common aneurism needle.

II. *Amputation at the Ankle Joint by internal lateral flap.* (*Edinburgh Monthly Journal*, August 1849.)

In this paper he proposes new lines of incision for Mr Syme's operation at the ankle joint, as applicable in all cases, but necessary in some in which the ordinary method cannot be followed. This proposal is not put forward as constituting a new operation, but as another method of securing what essentially constitutes Mr Syme's operation at the ankle, viz.—the employment of the natural cushion of the heel as the part on which the weight is to be rested, and the division of the bones through their cancellated texture immediately above the malleoli. He did not propose his method as one to supersede that of Mr Syme, but says, "the incisions now recommended and practised by Mr Syme will, in the majority of cases, be found best calculated to ensure the formation of a good and useful stump;" and three years afterwards he again refers to Mr Syme's method as, "in the majority of cases, the preferable proceeding." This new modification of it he proposed chiefly for those cases in which, from disease or injury of the soft parts behind the heel, the other method was inapplicable. Instead of preserving the cushion of the heel in a posterior flap by a perpendicular incision from malleolus to malleolus, it is included in a large internal lateral flap. The incision extends from the middle line behind the ankle to a little in front of the internal malleolus, the lower part of it passing along the outer side of the cushion of the heel. A semilunar incision across the outside, below the outer malleolus, connects the ends of the first incision. The woodcuts in Dr Mackenzie's paper illustrate the incisions. The large internal flap is easily dissected up, and the separation from the bone easily accomplished. In this method also he supposes, with some reason, that there is less likelihood of division or injury of the artery or arteries on the inside of the heel, while the danger of wounding the artery on clearing the lower end of the tibia remains the same in this as in any other method. The objection that in this method the vascular supply on the outer side of the heel is cut off, he very reasonably treats as of little importance, as the internal lateral flap includes within itself much the greater part of the vascular supply.

There is very little difference in the stump after the two methods, the cicatrix being in front in the one case, and towards the outside in Dr Mackenzie's method.

A similar method has been since recommended by M. Sedillot of Strasbourg, but the merit of originating this plan belongs to Dr Mackenzie.

The operation was often performed in this way by himself, and by other surgeons, with the most satisfactory results. In some of these cases, the state of the parts behind the ankle would have rendered amputation through the leg necessary, but for this modification of the incisions. A case in point we have just heard of from one of Dr Mackenzie's former assistants, now attached to one of the British hospitals in Turkey. Both feet were frost-bitten; on one side, the cushion of the heel was included by Mr Syme's method in the posterior flap; on the other side this could not be done, but, by adopting Dr Mackenzie's method, Dr Watson was able to give the patient the benefit of Mr Syme's operation on the other foot also, instead of having recourse to the much more dangerous mutilation of amputation through the leg—an operation which Mr Syme's ankle-joint operation has now nearly superseded in the Edinburgh school.

We therefore reckon Dr Mackenzie's method of performing this operation as a valuable contribution to surgery, as in ordinary cases the next best method to that of Mr Syme, and as occasionally the only one which is applicable or safe.

III. *Case of Stricture of the Urethra, in which the operation of division of the stricture by external incision was followed by a fatal result.—(Monthly Journal,* March 1851.)

The operation was performed in the usual manner, and the patient did remarkably well till the third day, when he had rigors, and became collapsed; and these symptoms continued more or less till the eighth day, when he at length sank. On examination, the parts at the seat of operation presented no unhealthy appearance. A small abscess had formed behind the bladder, and the right pleura was found thickly coated with lymph, and its cavity full of pus. Corpuscles resembling, or identical with, true pus corpuscles, were found in the blood in large numbers. The patient, therefore, as Dr Mackenzie concludes, died of *pyæmia*, which it is well known may follow any operation, and is beyond the control of the surgeon. The case is related with great minuteness and care. This operation had already been attended by remarkable success in the practice of Mr Syme, to whom we owe also this improvement in surgery; and, although it was the first instance in which Dr Mackenzie had performed it, he remarks, with characteristic courage and judgment, that the result "will not deter me from repeating the operation in similar cases."

IV. *Cases of Stricture of the Urethra, treated by external incision.*

This paper was published in the *Monthly Journal* for July 1853, but we notice it now in connexion with the preceding one. It contains the details of *seven cases* in which the operation was performed, being "the only cases in which I have found it necessary to depart from the usual treatment by bougies." In all the relief was immediate, and the result most successful. He observes—

> "Having witnessed the success which had followed the operation in numerous cases in Mr Syme's hands, this accident occurring in the first case in which I had followed the practice had no effect in shaking my confidence in the merits of the operation, and I have not hesitated since to perform it in every case of obstinate stricture which has come under my care, in which the use of the bougie has failed to effect a cure. In no case have I met with the slightest difficulty in the performance of the operation, nor anything to occasion a moment's uneasiness as to the result, and I have not performed the operation in any case in which the use of the bougie appeared to me to hold out any prospect of curing the disease." And again, "of all the cases in which the operation has been performed in Edinburgh, by Mr Syme and others (nearly a hundred in number), my own case, already referred to, is the only one, so far as I can learn, in which a fatal result has followed the operation."

V. *On Restoration of the Upper Lip.—(Monthly Journal,* October 1851).

In this case, the plan of restoration of the upper lip previously adopted by other surgeons—that of bringing a flap from the cheek—could not be carried out, as the patient, a girl eighteen years of age, had, when a child, lost the greater part of the right cheek, nearly the whole of the upper lip, and the entire nose, from mortification following fever. Dr Mackenzie conceived the ingenious plan of "*transposition of the lower lip to the situation of the upper*, whilst, at the same time, by extending the incisions under the base of the jaw, the integuments in that situation might be brought up to replace the lower lip, according to the plan recommended and successfully practised by Mr Syme." Both steps were accomplished together and successfully, and ten weeks afterwards the nose also was restored, by bringing a flap from the brow, which was likewise successful. Sketches are given of the appearance of the patient before operation, without either nose or upper lip, and then three months after the last operation; the horrible chasm is replaced by a most respectable lip and nose, highly creditable to the art of surgery and the ingenuity and dexterity of the operator.

VI. *Case of Restoration of the Upper Lip, Cheek, and Eyelid. (Monthly Journal,* January 1852).

In this case there had been extensive sloughing after fever. "Nearly the entire cheek and lower eyelid, the right side of the nose, and the right half of the upper lip, being destroyed. Necrosis of the exposed bones followed, and the nasal bones, along with the greater part of the right superior maxillary, were detached some time afterwards."

The restoration of the upper lip was effected as in the preceding case. Although the first attempt was defeated by non-union, the flap was again brought up some time after, and "perfect union was obtained, and the natural appearance of the upper lip was thus nearly quite restored." Three months afterwards, the deficiency of the cheek, eyelid, and side of nose, was supplied by a large flap from over the ramus of the jaw, and the flap united favourably. The woodcuts show a very great improvement on the child's face. The result in this case also is highly creditable to Dr Mackenzie, and we recommend the study of the details of these two cases to those who intend to perform plastic operations on the face.

VII. *Cases of Lithotomy.*—(*Monthly Journal,* January 1852).

The first case is a curious one. It is entitled, "Five calculi removed by lithotomy, each containing a *field bean* for its nucleus."

> "The lateral operation was performed, and five stones were removed. No difficulty occurred in the performance of the operation. The prismatic shape and uniform size of the calculi were remarked at the time as being curious, but the presence of a foreign body as a nucleus of each was not suspected till some days afterwards, when the stones and their nuclei having been deprived of their moisture by evaporation, a hard substance was felt and heard rattling loosely in the interior of each on shaking them in the hand.
>
> "On sections of the calculi being made, the nuclei (of the introduction of which the history has since been elicited) were revealed. The incrustation was entirely composed of the triple phosphates. The sections of the calculi are well represented in the woodcuts.
>
> "The patient made a speedy recovery."

The second case illustrates well the frequent history of the mulberry calculus. The presence of the calculus had been ascertained fourteen years before, but the symptoms began to abate, and for all this time the patient was entirely free from them. Under a new exciting cause they returned. A good-sized mulberry calculus was extracted without difficulty, and the patient made an excellent recovery.

VIII. *On the use of Chloroform in the treatment of certain cases of Stricture of the Urethra.*—(*Monthly Journal*, March 1852).

Two cases are related showing the great benefit resulting from the use of chloroform in cases of retention from *spasmodic stricture.* In both the attempts to introduce the catheter had failed, and in both the use of chloroform alone at once enabled the patients to empty the bladder.

> "It is not," he observes, "my wish to advocate the employment of chloroform as a general rule, even in those cases of stricture where considerable irritability of the urethra exists; but to call attention to the distinct proof of the constriction in this case being much increased by muscular spasm, and of this being at once relieved by the action of chloroform, as well as to the fact that the bladder retained its expulsive power, whilst the retentive muscular fibres were relaxed.
>
> "It is in the irritable form of stricture, accompanied with spasmodic contraction of the muscular fibres surrounding the urethra, that advantage is occasionally derived from the use of chloroform. In the treatment of ordinary stricture of the urethra, the use of chloroform is not called for. On the contrary, its employment, where no unusual irritability of the urethra exists, must evidently be attended with disadvantage, as the feelings of the patient form, to a certain extent, a guide to the surgeon in passing the instrument through the constricted part of the canal."

IX. *Cases of Rupture of the Urethra.*

In the same report as the preceding cases are two of rupture of the urethra, from injury of the perineum. They are very illustrative cases, one followed by extensive extravasation, requiring free incisions, the other not attended by extravasation; and both afterwards showing the strong tendency to contraction, or formation of stricture, at the injured part. The detail of the cases is followed by excellent practical remarks.

X. *Ligature of the Subclavian Artery.*—(*Monthly Journal*, February 1852).

This paper is entitled, "Case of aneurism of the upper part of the axillary artery attended by certain peculiarities, and unsuccessfully treated by ligature of the subclavian artery."

The doubt was afterwards raised whether this was a case in which the contents of the aneurismal sac had suppurated, or was originally one of abscess which had opened into the axillary artery. Since the occurrence of Mr Liston's well-known case, in which he believed the abscess in the neck to have at length opened into the carotid artery, it has been a subject of much difference of opinion among surgeons, whether this condition occurs, or has really ever occurred. Dr Mackenzie begins his paper with an examination of the facts on either side. After referring to the not unfrequent occurrence of an artery being opened into by ulceration or sloughing *after* the abscess has been laid open, he concludes regarding the occurrence of a communication with an unopened abscess, " the cases I have quoted seem to prove the occasional formation of this variety of aneurism." The doubt, he observes, arises chiefly from " the difficulty of proving the order in which the pathological changes in the cyst and in the coats of the artery have occurred; the difficulty, in other words, of proving that the cyst was not originally that of an ordinary aneurism, in which suppuration had subsequently occurred, and this difficulty may have given rise, in similar cases, to the true nature of the aneurism being overlooked."

He then proceeds to narrate his own case, which is one of great interest. The tumour in the axilla presented all the characters of aneurism in a marked degree, and there was no doubt in the minds of the surgeons who examined it that it was a case of the ordinary axillary aneurism. The subclavian artery was tied in the usual situation. " The operation was attended by unusual difficulty, from the number and size of the veins which crossed the triangular space." " The artery was, after some delay, reached and surrounded by a ligature, the sheath of the vessel having been freely exposed, but the coats of the artery denuded only to such an extent as to allow of the passage of the needle."

From this unassuming account of the operation it could scarcely be inferred that he was describing the successful accomplishment of the most difficult operation in surgery. It was performed in the theatre of the hospital, in the presence of several hundred spectators; and all who were present that day will recollect the dauntless courage with which, under these trying circumstances, he set about the operation, and with coolness and care conducted it to a successful close.

For some time all went on well with the aneurism and wound, but there had meanwhile been a severe attack of chest complaint. The ligature came away on the eleventh day, previous to which the wound had healed to the size of a pin's head. On the seventeenth day secondary hemorrhage came on through this minute aperture, and, after repeated recurrences, carried off the patient in three days.

On examination, there was found in the axilla a sac communicating by one, if not by two apertures, with the axillary artery, and containing pus and coagula of blood. Different opinions were formed and maintained as to whether this sac was a true aneurism within which suppuration had occurred after operation, or had been originally an abscess which had at length opened into the artery, and established this rare form of false aneurism. Dr Mackenzie gives a candid statement of the views on either side, and with apparently very good reason, but without dogmatism, concludes, that "an attentive consideration of these details can scarcely fail, I think, to lead to the conviction that the aneurism was of the variety described by Mr Liston; that it owed its origin to ulceration of the coats of the artery, which formed part of the walls of an abscess."

Whichever view was correct, there can be no doubt as to the propriety of the treatment adopted under the circumstances, and it is only to be regretted that it was attended by that result which so frequently follows the operation of ligature of the subclavian artery—the only means, however, by which the patient's life can be saved. Dr Mackenzie expresses doubt whether the ordinary Hunterian operation can be relied on with any degree of certainty to effect a cure of an aneurism previously ascertained to be of this nature, as the ordinary changes in the sac, and the artery near it, are unlikely to take place.

He says, " Were the character, then, of such an aneurism recognised during life, it appears to me that it would be better practice to treat the case like any other form of false aneurism—viz., by the performance of the old operation,—the laying open of the tumour, and the application of two ligatures, the one above, the other below the opening in the vessel. The prognosis, however, in such a case, under any circumstances, must be unfavourable."

It is worthy of notice that a very rare and interesting anatomical variety existed in this case.

" A remarkable variety existed in the position of the subclavian vein. It lay immediately below, and in contact with, the artery, and behind the scalenus anticus muscle. It diverged from the artery behind the scalenus to join the internal jugular vein. As far as I am aware, two cases only of this variety have been previously recorded" —one by Blandin, the other by Velpeau, to which he gives the references.

The preparation (which we have examined) shows the vein lying in this situation. It will be observed that the existence of this variety was detected only on subsequent examination. Although much closer to the artery than usual, and much more liable to be transfixed or injured, it was not noticed during the operation, and remained uninjured.

XI. *On Arterial Hemorrhage.—Cases of Ligature of the Femoral and Anterior and Posterior Tibial Arteries.*—(*Monthly Journal*, April 1852.)

The remarks on *Arterial Hemorrhage* refer chiefly to the circumstances under which we must depart from the usual method, as laid down and established by John Bell and Guthrie,—by the application of a ligature above and below the bleeding point,—and adopt the Hunterian principle in the treatment of false as well as true aneurism.

(1.) *Case of Hemorrhage from an ulcerated opening of the Popliteal Artery.—Ligature of the Femoral.*

The bleeding appeared three weeks after the abscess had been opened. The details show that the state of the popliteal space forbade any operation there. The femoral artery was tied at the apex of Scarpa's triangle. "The bleeding was at once arrested, but the patient, exhausted by the great loss of blood (about three pounds) which had taken place before the vessel was tied, sank, and died twenty-four hours after the operation. There was no recurrence of the bleeding subsequently to the application of the ligature."

Dr Mackenzie then refers to certain strictures made by Mr Guthrie on the practice he had pursued in this case of ligature of the subclavian artery for hemorrhage following the burn in the axilla. To these he replies very effectively, showing how ligature of the subclavian artery was preferable to Mr Guthrie's proposal, to secure the artery above and below the bleeding point, after dividing the anterior wall of the axilla. This is illustrated by diagrams of the anastomosing arteries between the subclavian and the axillary.

(2.) "*Case of Rupture of an Aneurism of the upper part of the Posterior Tibial Artery; Aneurism laid open, and the Artery secured by Ligatures above and below the opening in its coats.*"

In the detail of this case it is mentioned that after the artery had been tied both above and below the bleeding orifice, the blood flowed from it almost as before, though with less force. This was owing to the peroneal artery arising precisely here, and when it also was tied all bleeding ceased. The patient had been previously extremely reduced by loss of blood, but did well; and six months afterwards resumed his duties as a letter-carrier, "which he still performs in good health, walking not less than twenty-five miles a day."

(3.) *Secondary Hemorrhage following amputation of the Leg-Ligature of the Anterior Tibial Artery.—Recurrence of Secondary Hemorrhage.—Ligature of Femoral Artery.*

The patient was a very emaciated, unhealthy boy; and, in a convulsive fit, severely injured the stump fifteen days after the operation. When the bleeding occurred, the stump was opened out, and the bleeding point sought for.

> "A forcible jet of arterial blood issued from the situation of the anterior tibial artery. The bleeding point was distinctly exposed, but the artery gave way as often as it was seized by the forceps, and it was evidently necessary to place a ligature upon it at a higher point. In order to expose the vessel above the bleeding point, it was found necessary to disarticulate and remove the head of the fibula. This having been done, the artery was brought into view, where it passed through the interosseous ligament, and a ligature was placed around it at this point. Everything progressed favourably for a few days, but the ligature separated so early as the fifth day, and in five days more copious arterial hemorrhage again took place from the stump. A ligature was now applied to the superficial femoral artery, immediately above the crossing of the sartorius muscle. The hemorrhage was at once arrested, and never recurred."

Notwithstanding his feeble health, and these successive misfortunes, each requiring a new appliance of surgery, this patient ultimately did well.

XII. *Report of Surgical Cases.*—(*Edinburgh Med. and Surg. Journal,* January 1854.)

Our space will not permit us to do more than refer very briefly to these cases.

(1.) *Loose Cartilages in the Knee-Joint.*

Two cases are detailed—one illustrating the dangers of the former method of treatment, by direct incision and extraction; the second illustrating the safety and success of Mr Syme's new method of operation. Dr Mackenzie had come to an unfavourable conclusion regarding the plan of trying to push the loose body into the cellular tissue outside the joint, through an aperture in the synovial membrane, made by sub-cutaneous incision. Repeated and varied trials on the dead body, he states, had satisfied him of the great difficulty of making a sufficient opening in the synovial membrane, and of getting the loose body through it; but that this is now rendered unnecessary by Mr Syme's later method, by which the loose cartilage is fixed at the wound in the synovial membrane, at the side of the condyle, with the view of obtaining its adhesion to the surfaces of the wound.

2. *Dislocations.*

Some of the cases illustrate the rarer forms of dislocation. and others "the immense advantage gained by the use of chloroform as an auxiliary to the mechanical means employed in the reduction especially of dislocations of less recent occurrence." The cases are—

1. "Dislocation of the head of the thigh-bone on the dorsum of the ilium. Reduction ten days after the occurrence of the injury."

2. "Dislocation of the head of the thigh-bone into the sciatic notch. Reduction between five and six weeks after the occurrence of the injury."

3. "Dislocation of the head of the thigh-bone on the os pubis. Unsuccessful attempts at reduction nine weeks after the occurrence of the injury."

4. "Dislocation of the head of the humerus into the axilla." The reduction in this case was effected with remarkable ease, very moderate extension made with the heel in the axilla having proved sufficient. This he attributes to the use of chloroform. There is no other instance on record of a dislocation of seventy days' standing being reduced without considerable difficulty, and the employment of much greater force.

5. "Dislocation of the sternal end of the clavicle backwards. Reduction." This case is interesting from the great congestion of the face, and semi-comatose condition, produced by pressure of the dislocated bone on the great veins behind. These symptoms disappeared immediately after the reduction.

6. "Dislocation of the head of the radius forwards, of four months' standing."

7. "Dislocation of the head of the radius forwards. Reduction ten days after the occurrence of the injury."

(3.) *Rupture of Anchylosis by forcible extension.*

This is an account of a case in which anchylosis of the knee in the flexed position, following disease of the knee, had been broken up by a bone-setter. The lad had a narrow escape for his life, but a tolerably useful limb was obtained. A sketch of the limbs is given. Dr Mackenzie observes:—

> "But, although the result in this case has been successful, no one, I think, will have any doubt as to the means by which it was accomplished having been quite unjustifiable. My object in recording it is to shew under what adverse circumstances, both as regards the limb and the state of constitution, a limb *may* be restored to usefulness, and a recovery effected; and that a sound constitution, and in deformity of less marked character than in the above case, the breaking

up of an anchylosed joint may be undertaken with a reasonable prospect of success."

Amputation at the Hip-Joint.

The lad had suffered from necrosis of the thigh bone for several years, and, by an accident on the street, fracture resulted. On examination with the finger, the shaft of the bone was found to be so extensively necrosed as to render amputation advisable. The disease extended so high that Dr Mackenzie decided on disarticulation at the hip-joint, the propriety of which was afterwards more fully shown by examination of the bone. This was done by the antero-posterior flap operation. The limb was severed in less than ten seconds, and not a tea-cupful of blood lost.

> "The shock sustained by the system from this formidable operation was very slightly marked, a circumstance which I had previously had an opportunity of witnessing after the same operation in a similar case, and which was plainly attributable to the full influence of chloroform during the operation. When I visited him two hours after the operation, I found him smoking his pipe, and he expressed himself as highly satisfied with the relief he had obtained by the removal of the limb."

Every thing went on well for a week, when the first of several attacks of hemorrhage appeared, rendering it necessary to open up the flaps and secure three or four bleeding points in the posterior flap, but the patient sank in a few hours after this, notwithstanding that 14 oz. of blood were transfused into his veins.

Dr Mackenzie observes that "when the disease or injury admits of division of the bone below the trochanter major, disarticulation would of course be unjustifiable ; but when the saw must be applied higher than this point, I think it is more prudent at once to remove the entire bone, than to protract the operation even during the short time occupied in the use of the saw." "I cannot but think," he again remarks, "that this operation (sawing through the trochanter) is attended by a greater risk to life than the operation of disarticulation."

He concludes,—

> "Amputation at the hip-joint has been generally regarded as a desperate measure, and one from which the patient has a very small chance of recovery. Recent experience, however, has proved the operation to be attended by a less formidable amount of mortality than is generally supposed. Improved modes of operating, and a more judicious selection of cases, may so far account for the operation being in later years attended by more favourable results than formerly ; but a large measure of the recent success must certainly be attributed to the employment of anæsthetics, the effects of which

modify in such a marked degree the shock which followed the operation in former years."

In connexion with the causes of a fatal result from amputation of the hip-joint, we may here call the attention of surgeons to an important fact—that the operation has been four times performed in cases of *thigh stump* requiring removal, and was successful in all the four. Three of these occurred before anæsthetics were used. The difference between these and the other cases, is, that a much smaller part of the body is removed at the time. The extent of wound in both is nearly the same, though the hemorrhage must be less, and less likely to be renewed in the cases of amputation of the stump only. It is possible, however, that the explanation of the recovery in these cases may lie in the consideration, that a constitution which has enabled a patient to pass safely through a thigh amputation, is likely to bear him up in the lesser matter of the amputation of the stump.

XIII. *On Excision of the Knee Joint.*—(*Monthly Journal,* July 1853.)

The operation of excision of the knee-joint had long fallen into disuse, in consequence of the little encouragement received from the majority of the few cases in which it had been practised. Dr Mackenzie observes :—

> "Hitherto an impression most unfavourable to the performance of the operation of excision of the knee-joint appears to have been entertained generally by the profession ; and severe censures have been passed on those who have, within the last three years, revived the operation, as well by those who, practically, are quite ignorant on the subject, as by those whose experience and professional standing entitles their opinion to attentive consideration.
>
> "My own experience on the subject has as yet been too limited to warrant my speaking with any degree of confidence as to the result of the operation. I have performed the operation in two cases, but although the results of these cases hitherto have fully realized my expectations, yet the number is too small, and the time which has elapsed since the operation was performed is too short, to allow much weight to be attached to my personal knowledge of the operation. I have been induced, however, to write the following short notice of the subject by the conviction, from what I have lately seen of the results of the operation in several cases where it has been less recently performed, that the operation is one which has been rejected from surgical practice on insufficient grounds, and that it will yet come to supersede, in many cases, the severe operation of amputation of the thigh."

An abstract is given of the various cases in which excision of the knee-joint has been performed by other surgeons. Previous to 1850, the operation had been performed, he states, eleven times. Six of the patients had died, and five recovered.

> "Of the fatal cases, however, some at least may be said to bear little on the merits of the question of excision. Of the five cases which had recovered, the limb appears to have been thoroughly serviceable in three; the amount of usefulness in the limb in the fourth is unknown, and in the fifth case the limb was entirely useless.
>
> "In 1850 the operation was revived, and within the last three years the operation has, as far as I can ascertain, been performed thirteen times. Of the thirteen, three have died, two directly from the effects of the operation, and one from dysentery. The remaining ten are, as far as I know, at present alive. In five, the limb is already used freely in progression; in one, the result, as regards the usefulness of the limb, is yet uncertain; in one, I am ignorant of the result; in the remaining three, sufficient time has not elapsed to render the result certain, but of each the most favourable expectations may be entertained."

This account of the latter series of cases is given, he states, with three exceptions, "from my own personal observation of them." So much interested was he in this matter, that he even went so far as the island of Jersey to see the cases in which Mr Jones had performed the operation. "There," he goes on to say, "I had an opportunity of fully examining four of the patients on whom the operation had been successfully performed, and I cannot omit this opportunity of expressing my admiration of the skill and enterprise with which Mr Jones, in a small provincial hospital, has prosecuted this branch of surgery."

"Such is a brief outline of the history of all the cases in which, as far as I can ascertain, the operation has hitherto been performed, and I think I am justified in saying, that an impartial review of these cases, especially of the later series, at least proves that the propriety of the operation is yet an open question."

A special objection has been urged against the applicability of the operation in children—that the limb will not grow. Doubting this conclusion, in the absence of sufficient facts to establish either its correctness or incorrectness, Dr Mackenzie observes—

> "The limb in one of Mr Jones' cases had already, since the operation, grown considerably, and presented now no appearance of checked growth. As bearing on this question, I would be glad to know if the growth of the upper extremity is checked by excision of the elbow-joint. It is so, as far as I remember to have observed, only in a slight

degree. Surely the numerous children who have undergone this operation at the age of six or seven, have not grown to adult age with the stunted arm and hand of little more than an infant. Supposing, however, that the fact is so—that the limb is so much *respectively* shortened in after years—the objection applies only to the operation being performed on children, and has nothing to do with the question of the propriety of excision in the adult."

The patients were adults in both of the cases in which he performed the operation himself in the Edinburgh Infirmary. The results he regarded as very satisfactory.

In the *first case* the limb went on favourably, notwithstanding several dangerous complications—severe and protracted hiccup, diarrhœa from hepatic derangement, then, two months after the operation, a severe attack of pleuro-pneumonia, from all of which the man at length made a good recovery. "Six weeks after the operation the motion at the knee was very limited." Three months and a half after the operation (the date of Dr Mackenzie's description, which we quote) the patient "is still confined to bed, but this is a measure of precaution more than of necessity, to prevent any motion at the seat of operation till the bones are firmly consolidated. The limb, which is supported by a leather splint behind the knee, and a starched bandage, is slightly bowed outwards at the knee, and is one inch and a quarter shorter than its fellow. The bones at the knee are immoveably fixed as regards lateral motion, but on using a little force, slight motion backwards and forwards can still be perceived. Two or three weeks more, I have no doubt, will serve to complete the osseous anchylosis. He can move the limb freely in all directions, but is directed to keep it in as quiescent a state as possible."

We may add, Dr Mackenzie's expectations have been fully realized in the subsequent progress of this case. We learn by letter (of date January 4th 1855) from Dr Watson, formerly house-surgeon to Dr Mackenzie, that at the end of July 1854 (a year and a half after the operation), when casts of the limb were made, "at that time, indeed for two months before, the knee had been perfectly firm, and the cicatrix perfectly sound. *He could walk three miles without any fatigue*, and I daresay more, only he had no occasion to go farther. The shortening of the limb was between 1½ and 1¾ of an inch. I have heard from him since he returned home, and he says that the limb got stronger and more useful every day."

In the *second case* the operation had been performed only a fortnight before Dr Mackenzie's paper was written, but the patient was then going on very satisfactorily, and promising "as far as it has gone, a most favourable result." These expectations were verified. The patient left the hospital in three months, and has since sent accounts of his progress. The result is said to be as satisfactory as in the first case.

[It is proper here to mention that after his paper was written, Dr Mackenzie performed the operation in other two cases, one four months, the other one month, before he left for the seat of war. Both of these cases had a fatal termination, and it is right that we should mention them here in justice to Dr Mackenzie's memory, and that the statistics of this operation may be fairly reported. The patient in the first of these two cases was a boy (Miles Christie) æt. fifteen. The operation was performed in December 1853, and the patient sank about three weeks afterwards. In the other, Dr Mackenzie's fourth and last case, the patient was a boy æt. thirteen (William Cunningham). The operation was performed on April 15, 1854. The patient did well for six days, when he had rigors and chest symptoms, and died on the 26th, being eleven days after the operation. On examination the appearances of a severe attack of pleuro-pneumonia presented themselves, and both lungs contained tubercular matter in its earlier stages.]

Mode of operation. In the first case "the operation was performed quite according to the plan of Moreau, the patella being first removed, then the condyles of the femur, and lastly, the articular surface of the head of the tibia." The skin was reflected by an H-shaped incision, the connecting part of which passed across below the patella. In the second case, however, he improved considerably on this operation. One semilunar incision only was made, and the patella left.

"Having found the disadvantages of the H-shaped incision in my first case, I now exposed the interior of the joint by a semilunar incision, extending from the inner side of the inner condyle of the femur to a corresponding point over the outer condyle, the incision passing in front of the joint nearly as low as the tulcrosity of the tibia. The flap thus formed was dissected back, the *ligamentum patellæ* being decided, and the patella itself left in the substance of the flap. The rest of the operation was completed, as I have already described, with the important exception, that the patella was left in its place, and its immediate attachment left undisturbed. The cartilage, however, which remained on its surface was removed by the gouge, as well as the rough surface of bare bone around its articular margin."

"The semilunar incision seems to me the most advantageous, as being the smallest by which the joint can be satisfactorily exposed, and as giving rise to less bleeding than the H-shaped incision. The amount of bleeding was very trifling." He strongly recommends that none of the skin be removed, as, although apparently redundant at the time, there will not be more than enough afterwards. Although he did not remove any integument in either of his cases, "the retraction of the skin was such as to cause in each case, more or less gaping of part of the wound."

With regard to *not removing the patella*, he observes,—

"I have suggested that an error has till lately been committed in the performance of the operation, which has been allowed to interfere most seriously with its results. I refer to the removal of the patella. It is said that the patella is generally extensively diseased in affections of the knee-joint demanding excision or amputation. This, I have no doubt, is an error. The amount of disease to which it is liable is very limited; in the great majority of cases consisting only in its being more or less deprived of its cartilage, and of a rough, or perhaps carious, condition of its articular aspect. The patella is rarely, if ever, the primary seat of the disease in affections of the knee-joint. When the disease commences in the bones it invariably does so in the cancellated texture of the head of the tibia, or condyles of the femur. It is extremely rare that there is such extent of disease in the patella as to prevent its easy and effectual removal by the gouge, the body of the bone being left uninterfered with. The advantage of leaving the patella cannot fail, I think, to be very great. The natural form of the joint is preserved; the attachment of the extensor muscles is left undivided; the wound is less extensive; the annoying tendency to displacement forwards of the end of the femur seems in a great measure to be done away with; and there is every reason to believe that the consolidation of the bones will proceed more rapidly, and the limb ultimately be found both more useful and more seemly than when that bone is taken away. This is a point, however, which remains to be proved. In the two last cases, however, in which the operation has been performed by Mr Jones and myself, the patella has been left, and the advantages of its having been retained are already apparent in each case."

In regard to the choice between excision of the knee-joint and amputation, he observes,—

"The propriety of attempting to save a limb by excision of the knee-joint seems to depend on the three following considerations:—1st, Is the operation of excision attended by greater or less danger to life than amputation of the thigh? or, may the dangers of the two operations be considered equal? 2d, In the event of recovery after excision of the knee-joint, is the limb more or less useful and seemly than a wooden leg? 3d, Does the long confinement to the horizontal posture, which is necessary after excision of the knee-joint, in any measure counterbalance the benefits of the operation (if such benefits are proved), as compared with amputation of the limb?"

"A careful consideration of these questions, and of the recorded results of the operation, led me several years ago to doubt the propriety of the operation having been rejected from surgical practice. I satisfied myself, by repeated trials on the dead body, that the operation could be performed without difficulty, and without the risk of wounding any important parts; and, as a teacher of surgery, I have been in the habit of mentioning to my pupils my impression that the operation had been rejected on insufficient grounds."

In his remarks on each of the three above points Dr Mackenzie does not, in regard to the tediousness of recovery, admit it to be proved as greater in excision than amputation, at least that the limb is likely to be as soon fit for service as a stump after amputation usually is. In regard to the utility of the limb in the event of recovery, he decides strongly in favour of the stiffened limb in preference to a wooden leg; and as to the relative danger, after giving various reasons in favour of the excision, he concludes,—

> "Time and experience, however, can alone determine the comparative danger of the two operations. In the meantime the results of the limited number of cases in which the operation has been recently practised prove, as far as they go, that the danger to life is not great, and that it is inferior to that of amputation."

The operation of excision of the knee-joint is one regarding which much difference of opinion was expressed at the time when Dr Mackenzie brought it into notice. We therefore avoid expressing any opinion, but in this notice have endeavoured to give the facts, and his opinions, as given by himself. He has at least the merit, in the face of a general prejudice, of having thought and decided for himself, and of having courage to act on that decision. Whether he was too sanguine in his expectations regarding this operation, future experience, gathered partly from the stimulus of his example and recommendation, alone can show; but we may safely go along with him when, in conclusion, he says,—"My chief object at present is to present a summary of the cases in which the operation has been recently performed, and the results of these cases as far as they have gone, and I have endeavoured to do this faithfully and impartially. From what I have myself seen, I am satisfied as to the utility of the operation, and I think that the details which I have given of these cases prove that the operation, till its recent revival, had been banished from surgical practice on insufficient grounds, and that the propriety of practising it in certain cases is still at least an open question."

We shall close this notice with a few remarks on

Dr Mackenzie's Character as a Surgeon and Teacher.

His first field of public labour was in connection with the New Town Dispensary. Besides taking the surgical cases here which his colleagues at this institution entrusted to his care, he also did his share of duty as ordinary medical officer, which entailed much labour among the poor. Dispensary practice has but little professional attraction for one who has already had a complete practical education. A large proportion of the patients are brought there as much by want and misery as by disease, and the medical officer's duty often consists as much in deeds and words of kindness and benevolence as in the prescription of medicines; and hence, perhaps, it is that dispensary practice is satisfactory only to those who recognise the pleasure of doing good as at least in part the reward of their labour. We learn from those who knew Dr Mackenzie better in this position than we did, that this character attached to him in a high degree—the friend of the poor, as well as the physician of the sick.

We are better able to speak of him as a lecturer. He was satisfied of the necessity of becoming a teacher of surgery as an indispensable condition of eminence in it, and was ready to embrace the first opportunity. He delivered his first course of lectures in session 1849–50, as successor to Dr Argyle Robertson. His career as a lecturer, which extended over a period of five years, was decidedly a successful one. A teacher's popularity with the students is a good test of his ability and efficiency; for although they are but learners, and may therefore be supposed not to be capable of judging in such a matter, they nevertheless soon come to a correct conclusion. They may be deceived a little at first, or uncertain, but they soon find out whether they are being well taught or not, and if not, their attention and attendance flag, and next year they neither come back themselves nor recommend their friends, and the class falls off. But Mackenzie's class was good from the first, and continued to increase each year; and there is the more merit in this in a school where there is no imperative attendance, so that the numbers depend in great measure on the activity and efficiency with which the class is taught. Moreover, he began to teach at a time when for some years it had been little the custom of students to leave the university for their lectures. He was greatly liked by his students, junior and senior alike. In a class, like that of surgery, which embraces those attending for the second or third as well as for the first time, it is no easy matter to please all. Young students complain that they cannot follow some teachers, whilst, if the lecturer has only a routine, the more advanced students naturally weary of listening to just what they heard before. The difficulty is overcome by the lecturer explaining everything from the beginning onwards, recollecting that he is addressing many who are quite ignorant; and if he has the command of mind and matter enough of his own, to give the senior students something more than the bare elements and rules. Mackenzie had the admiration of all; the beginners were pleased with his clearness and simplicity, while the more advanced saw in him a thorough knowledge of his subject, as well those excellencies which cause us to like a man as often as it is expressed. At the same time, there is no doubt that the performance of difficult operations in public is the most trying position which a young surgeon can be placed in, the most difficult if not the highest exercise of his art; and when he does it unmistakably well, he is most deservedly held in reputation for it; he has shown, at least, that he can operate well, and he is not the less likely to be also a good surgeon on that account.

No one, however, was more fully impressed than Mackenzie with the belief that a surgeon may be a good operator and yet not a good surgeon—that a surgeon's calling is more to save limbs than to cut them off. Those who followed him to the wards and the waiting-room witnessed his skill in diagnosis and treatment. In beginning one of his papers, he refers to a regulation of the hospital, by which the junior surgeon is not allowed to give clinical lectures; but for this he made ample amends in the wards by the "clinical lessons" which he delivered at the bedside, commenting on the history, symptoms, and treatment of each case, the students having assembled around the bed in an orderly manner. This is in no way incompatible with the interest of the patient—the interests of both patient and medical science are bound up together. Were a surgeon to handle a fractured limb too much, to demonstrate the crepitus to the students after he was himself satisfied, or to allow his students to handle it for themselves after he was done, then he is not teaching surgery, but doing what he ought to teach them never to do. This, Mackenzie never forgot; the patients were his first consideration, and were always treated with kindness of manner and gentleness of hand; and, after this, came science in the shape of his "clinical lesson."

As a surgeon Mackenzie was decidedly a "practical" man in the true sense of the term. Moving in the centre of a great medical school, where all the accessory branches of the science have their ardent cultivators, by whom and through whom what is practical is so apt to be mixed up and confounded with what is not so, he was always able to distinguish what was practical, and left the other behind him when he approached the bedside, as matter of no interest to the patient. He had a clear head, and abounded in that common sense which seems to form so large and necessary an element in the character of a surgeon;—seeing at once what was wrong, knowing always what to do and how to do it, from his own resources. His experience in surgery was great for so young a man. A large part of this he had acquired under Mr Syme, and he had also seen much surgery in the continental schools. Few, perhaps, at his age have seen more surgery, or been so fortunate and successful with impertant cases. His opportunities had been excellent; but opportunities alone cannot make a surgeon, and did not make him. The opportunities he had were sought and followed after and wrought at, and by his industry and ability he had converted them into experience. Mere opportunities will not qualify for the arduous duties of the lecturer and hospital surgeon, much less carry a surgeon through these, in a trial of years, with distinguished success. Surgery admits of experience in it being acquired early, much more so perhaps than medicine. While the same individual must always grow in experience and knowledge as his years and opportunities increase, we may yet adopt Mr Liston's remark, and quote Dr Mackenzie as an illustration of it, that years are not the measure of experience, but he may be said to be the most experienced who has collected and arranged the greatest number of well-assorted facts to serve as precedents, whether these have been culled in five years or in fifty.

Mackenzie was a man of high spirit and honour and of independent mind. Although naturally gentle, no one could be more indignant than he at all instances of unworthy conduct, and he sought nothing that could not be attained by honourable means. If he sought, as we all do and should, reputation, he had no desire for notoriety. He did not revive old operations, or propose new ones, merely to attract notice, but was sincerely desirous of advancing surgery; and he had that independence of mind which left him unfettered by the opinions of others, and enabled him to think and decide for himself, as his writings amply attest.

But we must draw our remarks to a close, as we have far exceeded the space allowed us even by our indulgent editor.

We have spoken of what he accomplished during the latter years of his short life; and it is not too much to say, that had he been spared, he would have soon reached the very highest eminence. Eminence he had already attained, but of that kind which is only a sure stage to the highest. But it was otherwise ordered, and we can only now lament his loss as one who would have become a still greater ornament to his profession, and as a valued and attached friend. His death was sad news to his professional brethren, to his fellow-lecturers, to his students at Surgeons' Hall and at the Infirmary, and to all who knew him; and the circumstances under which it occurred painfully increased the feeling with which at any time his large circle of friends and admirers would have regarded his loss.

His fellow-lecturers met and expressed their sense of their loss in these terms:—

"At a meeting of the extra-academical medical lecturers of

Edinburgh held here to-day, it was unanimously resolved to put on record an expression of the deep regret with which they have learned the untimely death of their late colleague, Dr Richard James Mackenzie.

"Esteemed by them for his skill and accomplishments as a practical surgeon and teacher of surgery, and endeared to them by his upright character, obliging disposition, high spirit, and frank, open, honourable nature, they looked forward with pride and pleasure to welcoming his return from the scenes of warfare in which he was so nobly and usefully engaged. This expectation has been suddenly and mournfully disappointed.

"It is a consolation to them to know, that throughout a protracted and harassing campaign, in which he underwent much toil and hardship, their late colleague won the admiration and gratitude of all his comrades by his courteous demeanour, cheerful courage, conspicuous medical skill, and great kindness to the sick and wounded.

"The thought of this—mingled as it is with the feeling that the noble example which he set his brethren, and the honour which he has reflected on the whole medical school of Edinburgh, cannot receive from him any further exaltation or extension—but increases their sense of the greatness of their loss, and adds to the sorrow with which they give this imperfect but sincere expression to their grief at the early death of one so worthy of love and esteem."

Shortly after a meeting took place of medical men and students to take steps for erecting an appropriate monument to his memory. A marble bust was decided on, which has been very successfully executed by Mr Brodie, and will be placed in one of the institutions with which he was connected.

To praise the dead can do good only to the living. In writing some parts of this memorial we have kept in view that it would meet the eye of many of the students who knew him, and to them we have one word to say. You all liked him, and lament his loss, and respect his memory, and well may you do so; but do not stop here. Try to follow the example he has set you. He also was not long ago a student; and the example his life sets you is that of industry and honour and manliness, the doing good to others, and the faithful discharge of all the duties of life.

It is with regret that we now take leave of him. He is still as familiar to us as the evening we shook hands with him and wished him a good journey and a safe return. He well deserves to be long remembered in the medical school of Edinburgh; and of those of whom death has deprived it, either now or for a long time back, there are none whose memory will be more cherished than that of Mackenzie.

PART II.

CRITICAL ANALYSIS.

ART. I.—*A Practical Treatise on the Diseases of the Eye.* By WM. M'KENZIE, M.D., Surgeon Oculist in Scotland in Ordinary to Her Majesty, &c., &c. Pp. 1107. London: Longman, Brown, and Co. 1854.

Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.S., F.R.C.S., &c., &c. London: John Churchill. 1852.

Iconographie Ophthalmologique, ou Description, avec Figures Coloriées des Maladies de l'Organ de la Vue. Par J. SICHEL, Docteur en Médecine, &c., &c. Paris: 1852.

The fourth edition of Dr M'Kenzie's Practical Treatise on the Eye having been forwarded to us by the publishers, and a somewhat similar compliment having been proffered on the part of Monsieur J. Sichel from J. B. Baillière, we feel called to direct the attention of our readers to these very able and splendid works; to which we add the superb volume of the late Mr Dalrymple, whose brief professional career, though sufficiently ample to establish his own fair fame, cannot but be regarded as too short in reference to his ingenious labours and his distinguished acquirements. And here we should not neglect to mention that the planning of this splendid production was the work of the late Mr John Scott of the Royal Ophthalmic Hospital. "He," writes Mr Dalrymple, "conceived the plan, and purposed to give his collection of drawings to the public in a somewhat similar form to that now presented; but time was not permitted to him, and he bequeathed to his friend his valuable series of drawings, which, united with those collected by the latter for many years, have furnished a large stock whence the actual plates have been selected." 'Tis now going on to forty years since we had the felicity of attending the Ophthalmic Institution above alluded to, then superintended by John Richard Farre, William Lawrence, and Benjamin Travers; and it is somewhat singular that whilst those three celebrated men still remain amongst us, rich in years and honours, their successors—all, too, eminent men—Frederick Tyrrel, John Scott, and John Dalrymple, should have passed that bourne whence no traveller returns. At this time, too, we had the pleasure, in the great metropolis, of associating with Dr M'Kenzie, and have since rejoiced in the distinguished career he has run, and the many boons he has conferred upon this department of the profession. None of his compeers, we verily believe, either at home or abroad, have done more for the science; and high is the professional distinction which, by his steadiness, he has thus achieved for himself. It must afford no small gratification to the author, that three large editions of his original work have been exhausted, that it has been reproduced by a transatlantic press, and has been deemed worthy of being translated and published in the three best known languages of modern Europe —German, French, and Italian. Such a reception affords assurance that the labour of many years has not been bestowed in vain. And as to the present edition, we can truly affirm that a large amount of new matter has been added, and a faithful and laborious attempt made, so far as the author's abundant opportunities have served, to notice every material advancement in the pathology and treatment of the diseases of the eye, which has been made since the issue of the previous edition, now fourteen years ago.

In our limited space it will not be expected that we shall attempt anything like even the slightest sketch of a treatise which extends beyond 1100 pages of small pica and bourgeois, which is moreover illustrated by 140 woodcuts, executed in a most efficient and creditable style. All that we even aim at is to supply a few specimens of the new and original views, and it may be diseases, to which the author's attention has been directed, evincing at one and the same time the acuteness, clearness, and sagacity of his instructions.

In Chap. I., "Diseases of the Orbit," the following complaint is brought under review :—

Cysts in the Parietes of the Orbit.

"The bones are subject to two kinds of encysted tumours, viz.t he hydatid-encysted, containing echinococci, similar to those met with in the liver; and the serous-encysted. These tumours are developed in the cancellated structure, and expand the affected bones often to a great size. The frontal and the upper maxillary bones have not unfrequently been found the seat of such diseases,—in the latter situation connected sometimes with the fangs of the teeth. A remarkable example of hydatid-encysted tumour of the frontal bone is related by M. Keate. The tumour projected from the forehead chiefly over the left orbit, and presented the shape and size of three-fourths of a large orange. The tumour was laid open, and ultimately about twenty-eight hydatids came away. Twenty years after the last operation the patient continued well. In both kinds of the encysted tumour the bone covering the cyst must be freely laid open, and the cyst thoroughly extirpated."—(P. 51.)

In Chapter IV., "Diseases of the Tunica-Conjunctiva," another ail is added to those which had been previously catalogued, viz., lachrymal calculi in the sinuses of the conjunctiva. A single sentence or two will elucidate the subject:—

"Some years after the patient was again attacked with the disease. Concretions of the former colour, size, and other properties, formed in the left eye; at first they lay between the eyeball and under eyelid, and afterwards between the eyeball and upper eyelid. In the course of a few days the formation of calculi began in the right eye. On this occasion both eyes were less severely inflamed, and the disease was likewise shorter in duration. Walther immediately ordered her the solution of potash. The number of calculi, which were daily generated, soon diminished, and the whole process ceased in a short time."—(P. 235.)

The following paragraph in Chapter XII., "Foreign bodies in the vitreous humour," merits attention.

"Foreign bodies, such as fragments of glass, wood, iron, &c., may be driven into the vitreous humour either through the cornea and pupil, wounding and displacing the lens in their passage; through the cornea and iris, lacerating, and generally wounding the lens; or through the sclerotica and choroid, in which case the retina is apt to be torn and destroyed. They are often sent with such violence into the eye that they sink entirely out of view. The patient is not conscious that anything is lodged in the eye; he merely supposes it to have been cut or injured externally, and a hurried or superficial examination by the surgeon may not reveal the full extent of the mischief which has been inflicted. The blood effused into the eye in such cases is gradually absorbed, the inflammation is combated by the usual remedies, the wound contracts and heals, with the dragging, perhaps, or closure of the pupil, opacity of the lens, and invisibility of the retina. But the eye still continues irritable, the iris assumes a greenish or reddish hue, the sclerotica becomes thinned, and the eye soft and atrophic. By and by the eye may become the seat of acute pain, which sometimes intermits for weeks or months, and is then renewed with very painful severity. At length the wound, which had previously been cicatrized, opens, and allows the foreign substance to protrude. It is drawn forth, and often proves of a size so large as to excite the wonder of all concerned how it had lain so long concealed within the eye, or how it had been capable of lodging there at all. Under such circumstances, sympathetic ophthalmitis of the sound eye is apt to be excited during the pressure of the foreign body in the eye, or after it is extracted.

"In severe wounds of the eye, in which there is a suspicion of a foreign body having passed into its interior, a careful examination of the injured organ should be instituted while the patient is under the influence of chloroform. The immediate extraction of the foreign body will not merely prevent a great amount of immediate suffering from the state of the wounded organ, but may be the means of saving the sight of the sound eye from being lost from sympathetic inflammation."

In the Thirty-ninth Section of the Thirteenth Chapter, the subject of INTERMITTENT OPHTHALMIA is handled; and we meet with the following remarks:—

"At the period," says the author, "I sent to the press the earlier editions of the work, I did not think there was sufficient ground to admit the existence of any disease of this kind so regularly periodic in its accession as to warrant the appellation of intermittent ophthalmia, although I have since seen cause to change my opinion. The cases here recorded sufficiently, it would appear, settle the point. We extract the shortest of them:

"A lady of rank, who each summer, for two years, had taken large doses of sulphate of quina for hay-fever, became suddenly affected with dimness of sight in the right eye, followed by external appearances of inflammation. When she came under my care, in May 1842, the attacks of inflammation involved chiefly the retina, iris, and anterior crystalline capsule. The pupil was contracted and irregular, the iris was discoloured, and its edge appeared thinned, and was studded with minute reddish points. These symptoms, as well as the dimness of the sight, yielded to leeching, calomel and opium, and belladonna. After being some short time under my care, I discovered that the disease suffered a distinct exacerbation every seventh day. Each paroxysm began with slight redness and œdema of the conjunctiva, followed by more or less pain in the eye, generally nocturnal, and by an increased dimness of sight. These symptoms went on increasing for two days, and then subsided till the seventh day, when they regularly returned, often with a slight rigor. These relapses were in a great measure warded off by small doses of sulphate of quina.

"In December, the same set of symptoms occurred in the left eye, with tags of the pupillary margin to the capsule, and were overcome by the same remedies as had before been used for the right eye. On the tenth day of the treatment, the tags gave way, and the eye was free from redness, and seemed perfectly well. Next day the patient had a rigor, followed by pain in the eye, with redness and slight œdema, and the pupil again showed itself irregular and contracted. By means of calomel, with opium, and the belladonna, the eye was again restored, and the patient was put on sulphate of quina previously to leaving Glasgow. Two years afterwards, I was sorry to learn that the disease had returned, and had ended in amaurosis. This disease was evidently of chronic intermittent ophthalmia, of iritic character, and tertian type."—(P. 624.)

The following note upon "chronic vesication of the cornea" is worthy of remark :—

"I have had occasion to notice that the epithelium of the cornea is sometimes separated from the anterior elastic lamina by the intervention of a watery fluid. I have seen this happen without any previous inflammation of the eye, and with the cornea perfectly clear; but in general there is more or less opacity, and the eye has suffered long and severely from one or other of the ophthalmiæ. Falling into folds, on the lids being closed, the loose epithelium causes the feeling as if some foreign body were in the eye. Puncturing the epithelium, puncturing the cornea, and exposing the eye to the vapour of hydrocyanic acid, are found useful in this affection, along with the internal administration of tonics."—(P. 690.)

Possibly at the present time the palliative and medical treatment of CATARACT is not so generally attended to as it ought to be. At all events the following lines should be read with consideration :—

"Shading of the eyes and dilatation of the pupils by a drop of the solution of atropine every second night constitute the palliative treatment of cataract. Not merely in the incipient stage, but even in the advanced, many patients find their sight so much improved by these simple means, that they defer, for months or years, to submit to any surgical operation . . . . Bloodletting and mercury are likely to be attended with good effects, if inflammation is the cause of the opacity of the lens and capsule. The efficacy of these remedies in incipient spurious cataracts, especially those of the fibrous kind, is fully ascertained; but in true cataract they are seldom or never tried. Yet in certain cases of this sort they might perhaps prove beneficial; for instance, in the very commencement of Morgagnian cataract, a disease which, according to Beer, results chiefly from external irritation."—(P. 766.)

Such notices, relating to subjects new and old, might be added almost without limit, and to the profit of the student; but our space is well nigh exhausted, and we must meanwhile bid adieu to our able guide. We shall do so by giving a somewhat more detailed account of the author's graphic description of OCULAR HYPERÆSTHESIA, a fair specimen of his tact alike for observation and description :—

"The very remarkable state of disease which I purpose to designate by the name *ocular hyperæsthesia,* appears to consist chiefly in a greatly increased sensitiveness both of the optic nerve and of the ophthalmic division of the fifth. The chief characteristic of the affection

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is great intolerance of light, with which are combined, in a greater or less degree, photopsia, chrupsia, pain in the eye and head, augmented tactual sensibility of the eyeball and eyelids, and spasmodic contraction of the orbicularis palpebrarum. So far as the disease implicates the fifth nerve and the portio dura of the seventh, it appears to be the effect of a reflex action communicated to them in consequence of the excited state of the retina and optic nerve.

"The disease occurs both in an acute and in a chronic form, the former generally arising suddenly from some evident external cause, reaching its greatest degree of severity in a few hours, and, after some days, either yielding speedily to treatment, or rapidly subsiding of itself; the latter rarely traceable to any particular excitement of the organ of vision, but seeming to depend rather on the state of the constitution, gradually increasing in violence during a number of days or weeks, often continuing severe during many months, proving scarcely amenable to treatment, apt by its long continuance materially to compromise the general health of the patient, merging sometimes into a state which we might be tempted to call *ocular hypochondriasis,* but ultimately undergoing a perfect and almost spontaneous cure.

"In the acute variety, one eye is generally first affected; but by and by both became implicated. I have, however, seen the reverse of this, both eyes suffering at the commencement, while the disease concentrated itself speedily in one eye only. The attack commences with such excruciating pain in the eye as causes the patient to shriek aloud, and sometimes to fall suddenly to the ground. He is rendered totally incapable of exposing the eye to the light, and he immediately seeks to relieve this symptom by remaining in the darkest place to which he has access, pressing his eyes with his hands, or lying on his face in bed, and pressing his eyes against the pillow. The spasm of the orbicularis palpebrarum is such that it is almost impossible to obtain a glimpse of the eye. The pulse is full and quick, and the head hot. The symptoms are so severe, that the patient concludes his sight to be lost, and the practitioner, if unacquainted with the true nature of such cases, perhaps adopts the same idea, and pronounces the disease to be acute retinitis.

"Called to visit a patient with chronic ocular hyperæsthesia, we are probably ushered into a room as absolutely dark as it is possible to make it, every chink by which light might enter being closed, and a large screen so placed as to impede any gleam which might intrude on the door being opened. There we find the patient, in some cases sitting up, in others lying in bed, with his eyes shaded, and his head covered with a thick veil or shawl. In this state, we are perhaps informed, he has been for months. In such a case no persuasion will convince the patient that he might bear, for a few moments, sufficient light to enable us to see his eyes; or if he yields so much as to remove the coverings from them, his terror of their being touched with the finger prevents us from making any satisfactory examination of their state. The increased sensibility of the fifth nerve is a symptom scarcely less prominent in such cases than the excessive intolerance of light. The least touch about the eye is painful, and continues to be felt for a long time.

"However severe the symptoms, we may, both in the acute and in the chronic variety, promise a perfect recovery. In the acute, the recovery is generally sudden; in the chronic, gradual, but sometimes sudden and complete. The long duration of the chronic affection sometimes operates unfavourably on the nutrition and general strength of the patient, so that he continues feeble and reduced in body after his ocular symptoms are entirely subdued.

"Acute cases, from the violence of the pain by which they are attended, have generally been treated by depletory measures, such as venesection and the application of leeches, followed by calomel, with opium, and, after a time, by sulphate of quina. I have known a complete cure effected by this plan of treatment. In a severe case to which I was called, in which leeches and counter-irritation had been employed, neither the internal use of tincture of belladonna, nor fomentations with opium, were of any avail; but as soon as the mouth became sore from the administration of calomel and opium, the symptoms were overcome, and the cure was completed under the use of the citrate of iron and quina.

"The intolerance of light in both the acute and the chronic variety, yields for a time to the anæsthetic effect of inhaling the vapour of sulphuric ether or chloroform; and I have known the repeated use of this means produce permanent relief. The vapour-bath is a means which proves very beneficial in chronic cases. The re-establishment of the general health, after the more distressing symptoms have yielded, may be greatly promoted by change of climate, and especially by passing the winter in the southern parts of Europe.

"When the eyes are extremely intolerant of light, the best sort of shade is one formed of several layers of black crape or black silk, mounted on a light frame of silver or brass wire, embracing the upper half of the face like a mask, but sufficiently hollow not to come into contact with the eyes. In cases of less severity, shaded glasses may be employed, those for day use being neutral-tinted, those for night use blue."

We conclude this slight notice of a truly-admirable work with a cordial and earnest wish that the gifted author may long be spared, a credit to the profession, and a benefactor to suffering humanity.

II. We turn secondly to the splendid folio of Mr Dalrymple, the beauty and sterling worth of which cannot be considered as overcharged, even at the high figure which is indicated below.[1] This unquestionably will greatly surpass the limit of the hard-working apothecary, or general practitioner, who might occasionally be solicitous to revive his recollection of bygone and almost forgotten lore; the readiest remedy for which would be a reference to this splendid production, as found on the shelves of the public library, or in some professional book-club. Here, we doubt not, it would attract attention; and not so much from its inherent beauty and execution (by the pencil and delicate colouring of Mr William Bagg), as from the sterling instruction and assistance it affords throughout the wide field of ocular disorders. We have hinted above, that the original cases were mostly supplied from the Royal Ophthalmic Hospital, that innumerable sketches were taken under the auspices of the intelligent and devoted medical staff; and it is a selection of those rich and rare opportunities which have been seized, as it respects alike the history and the representation, and which is placed before the reader's eye. It was not the author's intention to publish a Treatise upon Eye Diseases, but rather to exhibit them as they occur in nature, in a series of drawings, and with such explanations as would identify them with symptoms, and with the general treatment of the case. Thus the whole consists of portraits of disease in various stages; and must aid those less familiar with the objective symptoms, in forming their diagnosis, when cases fall under their notice, and are confided to their care. The plan has long been followed in cutaneous affections; and the numerous splendid works that have emanated from the press of late years, indicate the value in which the profession hold such productions; and it is hoped that the work now presented may meet with a like success. It is adapted to accompany any systematic treatise on Diseases of the Eye that members of the profession may possess.

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[1] The sale price, half-bound, is £9, 15s.

Each plate exhibits on its surface, speaking generally, the correct portraits of six diseased eyes; sometimes more or less, in their different stages, and that at times essentially different. Thus we have a portrait of such complaints as conjunctivitis, pterygium, purulent ophthalmia, leucoma, staphyloma, &c., &c., immediately connected with which is letterpress to the extent of two or four folio pages, embodying the treatment which the author considers most judicious and successful, a couple of brief specimens of which we may supply, thus enabling the reader to form his own judgment of the work.

> "Iritis is the disease of the eye above all others interesting to the pathologist. Whether we regard it simply as affecting the iris, and endangering the vision, from closure of the pupil, as the type of an inflammation of a serous membrane, or as illustrating, in a far more perfect manner than any other human malady, the *modus operandi* of our remedies, it must be considered a subject well worthy the most attentive consideration and the profoundest observation.
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> "The first appearances of an eye affected by iritis is the faint pink

blush that more or less fully encircles the cornea, the seat of which is the sclerotica itself. This zonular form of vascularity is said to be typical; but, in strict truth, it rather indicates inflammation of the internal structures of the eye generally than of the iris in particular, for we find it in aquo-capsulitis, in choroiditis, in retinitis, and more or less in acute glaucoma. The form of the vascularity is zonular, not only from the aggregation of numerous red vessels that traverse the sclerotica at the fore part of the globe, before penetrating to the iris, but is made up of a series of straight vessels, all radiating towards the corneal junction, anastomosing but infrequently with each other, and hence easily distinguishable from the reticular form of conjunctival inflammation. It requires generally a good glass to distinguish any separate vessels, from their minuteness, and because they are but faintly seen through the tendinous aponeurosis of the muscles and the submucous tissue of the conjunctiva. The colour, therefore, at first is pink rather than red.

"The most essential point to be recollected by the surgeon in the management of a case of iritis, of whatever nature, is, that it is essentially an inflammation of a tissue covered by a serous membrane, placed in a cavity lined by a serous membrane, and in close proximity, posteriorly, with transparent bodies similarly indued. Let him also remember that the tendency of the disease is to contraction of the pupillary aperture. These facts constantly before him, he will be at no loss to conjecture the serious danger to vision that exists in the results of adhesive inflammation. In the contracted state of the pupil, the uveal surface of the iris is in such immediate vicinity to the more convex part of the anterior capsule of the lens, that lymph thrown out at any point of the uvea, and especially from the extreme margin of the pupil, gains ready adhesion to the capsule, inducing thereby fixed points that prevent dilatation, and giving rise to various irregularities and deformities of this aperture, or even to permanent closure, by exudation and organization of a plug of fibrin, rendering the capsule opaque, and constituting spurious cataract."

CATARACT.—The important subject of cataract is illustrated upon two plates containing twelve representations of as many varieties of the disease, and each highly characteristic. In this respect they are in the highest degree satisfactory. We give an extract or two upon this important subject.

"Fluid cataract is met with at all ages, but is a somewhat rare species. It appears to consist in a disintegration of the lens; and when it occurs in elderly persons, the nucleus of a hard lens is often found in the interior of the capsule. This latter investment may or may not be also opaque. When the capsule is entirely transparent, it will generally be seen on dilating the pupil, and if the eye has been a short time at rest, that the colour is not wholly uniform; that at the lowest part the opacity is denser, while a certain degree of milky translucency may be seen at the upper part. If the eye be suddenly rolled about or rubbed, the opacity becomes general and uniform, but clear at the upper part by rest and subsidence of the more opaque materials of which it will be found to be composed. It is by this means that we may distinguish a fluid from an ordinary soft cataract. On examining the contents of the capsule, when we have the somewhat rare opportunity of doing so, we find the fluid semi-opaque containing the debris of the lens, which may be easily distinguished by the microscope, also many oil globules, and sometimes plates of cholesterine."

A second extract, still more practical, we subjoin :—

"For the cure of soft cataract, whether congenital or not—capsular or lenticular—the needle only is required. In the great majority of instances, keratonyxis is the safer and better operation, especially where the lens exists in its entirety. The greatest mistake into which young operators fall, is the attempt to do too much at first; for, under the most favourable circumstances, the operation requires repetition, and most disastrous results follow from opening the capsule too largely, or disuniting the lens too extensively. The displacement of the lens, its dislocation into the anterior chamber, the swelling up of this body by the imbibition of the aqueous fluid, and its consequent pressure upon the iris, or even the displacement of large fragments into the anterior chamber, give rise to great embarrassments in the after treatment, to sickness, pain, and inflammation. Iritis follows, and the eye is not unfrequently lost, or adhesions of the pupillary margin are contracted, which greatly mar the final success.

"The first operation of opening the capsule should be specially limited to a small central aperture. It should be recollected that no displacement of the lens, or even fragment of it, should be permitted, by too large an aperture, until absorption have reduced its original bulk, and the admission of the aqueous fluid softened its texture. If this be duly attended to, each subsequent operation will be easy, more may be effected each time, and when the bulk of the cataract is greatly reduced, the capsule may be so far torn up as to present no impediment to future vision. If an operation be thus conducted, little after-treatment is required; no inflammation follows; the pupil is simply dilated with belladonna, and the patient recovers from each operation in two or three days."

One short extract more, and we must have done with these elegant, instructive, and highly practical pages. It regards strabismus :—

"In my own practice I never operate upon children under fourteen years of age. Where the operation does not fully succeed, it leaves the case worse than it found it, and in very young persons strabismus may be overcome by careful training, in some instances; and in others, it becomes spontaneously straight, either from improved health and tone, or from some of those changes that occur about the age of puberty."

III. Turn we now, finally, to the splendid and yet unfinished work of Mons. J. Sichel, a great folio volume of the same cast and character as that of Mr Dalrymple, like it consisting of coloured plates and letterpress, and which might excite a keen and critical comparison respecting their several merits and beauties. It is still issuing from the press, appearing in Parts, which reach us somewhat tardily; and any present attempt at a final judgment would clearly be premature.

The author informs us that the work is the result of more than twenty years labour, and that it has lain by him for upwards of a dozen of years. After numerous trials of art, he arrived at the conclusion that the highest style of *engraving* would best subserve his purpose. The plates, printed in colours, are retouched with the brush; and for the more minute details double plates are used,—the former for the principal parts of the designs, the latter for the finer and more accessory portions. As to the original drawings, he cannot well conceive—and we quite agree with him—that they could be exceeded. They are taken by M. Emile Beau, who devotes himself exclusively to M. Sichel's work, and up to the present time takes drawings of the more remarkable cases, whether they occur in his public clinique or in his private practice.

The splendid folio is divided into three parts—first, the letterpress, which has now reached to the 200th page; then the plates, of which the last is No. 28; and lastly, a careful explanation of the plates. The style in which these are got up generally resembles that of Mr Dalrymple's work, usually containing six representations of diseased eyes on each page, although the more advanced plates very frequently exceed this number. Plate No. IV. gives a very distinct exhibition of the microscopic tissues of the eye, according to M. Bruecke, exhibiting successively the string of *rods*, then those of the *nuclei*, the *cerebral cellules*, the *fibres of the optic nerve*, and the bounding *limitante*—the limiting membrane. Several plates are also devoted to accurate illustrations of the several instruments that are employed in the different operations upon the eye, and brought up to a late period, including those invented upon this side of the Channel, which must be useful to the parties interested in such matters. With respect to the drawings themselves, while we cannot venture to give any artistic opinion, yet to our taste nothing can well exceed them in accuracy, elegance, and beauty.

The difference in the general treatment of the subject by the Frenchman and the Englishman is sufficiently marked. We have previously indicated that Mr Dalrymple has represented his half dozen of drawings in his plate, and then in the accompanying letterpress gives a succinct account of all the disorders delineated. His space being limited, his reflections are correspondingly curt and axiomatic as well as important. The style of M. Sichel is very different. He does not limit himself as to space, and hence his disquisitions are not unfrequently minute, discursive, and extensive—a method which has its peculiar advantages, but is not so much desiderated by the practical man.

Perhaps the next circumstance in our review which most excited our attention was the free—not to say the extraordinary—use which the French oculist makes of bleeding, local and general. Passing by what in the earlier part of the present century was considered as the Egyptian or contagious ophthalmia, and where our treatment was, for a time at least, carried to a heroic pitch, many will remember the time when venesection was *largely* used amongst us for a series of years, and not only, as we supposed, without detriment, but with marked advantage. A great revolution, we need scarcely remark, has within latter years most assuredly occurred upon this side of the Channel, and that not in relation to the ophthalmiæ alone, but in many, not to say most, of the inflammatory complaints. So much is this the case, that it is no uncommon thing for the surgeon not even to carry his lancet about with him. One thing, however, appears very evident from M. Sichel's pages, to the effect that phlebotomy has not as yet lost its prestige in Paris, and this both as it regards local and general bleeding. In acute cases of simple, and even in scrofulous ophthalmia, our author advises that from four to twelve leeches should be applied in front of the ear (p. 96); and where the cornea is compromised, he advises that from four to twenty leeches should be applied in the same locality; and in cases of an attack of both eyes, the application should be renewed and phlebotomy had recourse to (p. 96). In his second chapter, on Traumatic Ophthalmia, we were surprised to see that our accomplished author advised the abstraction of some three cupfuls of blood, and the application of a dozen or a score of leeches near to the affected eye,—a practice to which at one time we somewhat approximated, but which now would generally be regarded amongst us as alike unnecessary and injurious. Lest the reader should imagine that we are somewhat exaggerating, we supply a few additional specimens. Regarding the treatment of rheumatic ophthalmia, the author advises that the patient should lose blood to the extent of from two to four cupfuls, and this should be repeated if necessary!! Leeches should then be applied to a number extending from eight to fifteen, and these followed by cupping at the nape of the neck, or the temples, the shoulders, &c.; which local bleedings should be repeated until the symptoms yield!! (p. 43.) It may be interesting for our author to know something of the extent to which venesection is discarded in this country, and perhaps more particularly in the treatment of eye cases than in those where the system is more generally affected; that a general bleeding is seldom thought of; that a greater number of leeches than two or three is but seldom ordered; and that the practitioner usually finds that when this is followed up by the administration of the tartrate of antimony, and other sedatives and curatives, relief is afforded, and the general health is not imperilled.

We had fully intended to bring various other portions of M. Sichel's work under the reader's notice. The extracts, &c., are before us, but we find that our allowed space is exhausted. At a future time it is possible we may return to this interesting work; and now, wishing the talented author a happy conclusion of his splendid task, we must for the present close.

ART. II.—*The Pathology of Drunkenness.* By CHARLES WILSON, M.D.

The period in which we live is strikingly and honourably characterized by the attention which is paid to the moral and physical condition of the humbler orders of society. Whether this proceeds from the increased benevolence of the more fortunate classes, or from the growing information of those below them, and their consequent determination to assert and enforce what they conceive to be their just claims, or from both combined, we do not at present inquire; the fact is certain, and cannot escape the notice of any one who bestows the most cursory attention on any department of our literature. Imaginative, scientific, and even strictly professional books, are full of references to the condition of what it is now the fashion to designate "the masses," and of suggestions for their improvement; and of all subjects of this nature, as none are more important, so there are none more intimately connected with medical science than those relating to the effects of intoxicating drinks on the human organization, and on the framework of society in general. The efforts that have been made to abate the gigantic evils resulting from the inordinate use of stimulating beverages, if they did not originate within the profession, would, we cannot help thinking, gain prodigiously, if they were guided and directed, and, on many occasions, controlled, by professional experience and sobriety of statement; for we feel well assured that these efforts have been not a little injured by the injudiciousness and extravagance with which they have been advocated. Nothing, indeed, can more strongly argue for the inherent strength of the Temperance cause, than that it should have continued to exist and make progress under such an incubus as the literature which professes to support it.

It is with the greatest pleasure, then, we receive a work like Dr Wilson's, which, in our judgment, exactly and with admirable ability and spirit, both expresses and exemplifies the part that professional men should take in the movement now going on in society. Though intended for popular circulation, this little book is strictly professional in its character. It has no resemblance to an exotic prize-essay, and is raised infinitely above what are called books of popular medicine, both by the purity of its aim and the high talent exhibited in its execution. It is not written in defence or in eulogy of any institution or plan, existing or proposed, to abolish or abate the evils of drunkenness. Allusion to these is but sparing, and extremely guarded. It does not seek to minister to the zeal of enthusiastic partisans; there is nothing in it whatever merely *ad captandum vulgus.* It is simply a statement of the effects of alcoholic stimulants on the human frame, from the first stage of their use as occasional means of convivial enjoyment, up to their last and fearful abuse as the instrument of habitual intoxication. But it is a statement which could have been given by none but a man of exact and careful observation, and, what is more rare and valuable still, by a man who can reason and reflect powerfully and correctly. There is also a rigid truthfulness in Dr W.'s descriptions, which makes them exceedingly impressive; and his conclusions are so calm and so clearly deduced from his facts, that every one who reads this book will acquire from it the necessary elements from which to determine for himself the line of conduct he should adopt both as an individual and a member of society. This, we think, is the precise position (and a most important one it is) that members of our profession occupy, when they come before the public on such a subject as we are now considering. But Dr W. is not only entitled to the station we have assigned him from powers of thought and observation, and the close attention he seems able to bestow on his subject, but from the extent of information he gathers from every quarter regarding it. The learning in this little treatise is great, and, highly creditable as it would be to a recluse student, is infinitely more wonderful in a man employed in active life. Highly as we estimate Dr W.'s talents, when we consider the unwearied perseverance and patient zeal which it must have required to amass the amount of information he possesses, amid the fatigues and distractions of country practice, we cannot refrain from offering him our very sincere and still higher admiration of his great and varied attainments. Having said thus much of the author, we proceed to give such an account as our space permits, of the book itself.

The first chapter professes to give the Medical History of Alcohol, to the effects of which, as forming the essential element of all intoxicating drinks, Dr W. exclusively confines his attention, and contains, besides, a brief but interesting account of the drinking usages of the ancients, from which we quote the following:—

> "It is to the credit of former civilization, that habits of drinking to excess never prevailed extensively among the more cultivated nations of antiquity. Although both the Greeks and Romans possessed wines of the most racy qualities, with the intoxicating effects of which they were, and could not fail to be, familiar, we have yet the fullest evidence that their manly sense rarely permitted them to degrade themselves by their abuse. We gather from their moralists, as well as from their poets and satirists, that the inebriety, which they did not fail to stigmatise, was merely the casual vice of individuals, and not the characteristic depravity of whole classes; and that to drink wine undiluted was, for the most part, accounted disreputable, while the degree of its dilution was usually very considerable. It is worthy of being noted, however, that those allusions to intemperance which we possess among the ancient writers become at once more distinct, and more extended in their application, as we approach the period of the declension of the general virtues, as well as of the pre-eminence of the Roman people. What Lucretius paints with a few graphic touches in the individual, Seneca shows more plainly in certain of its closer relations to society; while, in Juvenal, if the picture be less minute, and its traits be less directly grouped, they have become meaner and more loathsome.
>
> "But what is more to our present purpose, the medical writers of Greece and Rome, distinguished as they were, especially the former, as accurate observers, dwell little on the subject of intemperance and its influence in the production of diseases; although a close attention to the effects of diet and regimen justly occupied a principal place in their recorded observations. It is true that Hippocrates, in his second book on diseases, which is, however, of doubtful authenticity, appears to distinguish intoxication as the efficient cause of a peculiarly fatal form of apoplexy; and, in the fifth section of the fifth

book of Aphorisms, he speaks of the danger of convulsions which may arise from a similar source: an observation which has been transferred by Celsus to the second book of his celebrated treatise. But even if we add to these the case of Chærion, which is the fifth of those narrated in the third book of Epidemics, and the features of which suggest to us its identity with the delirium tremens or shaking delirium of drunkards, we have still, in a series of facts so scanty, only a further proof of the slight importance which the habits of the times permitted the medical observer to attach to intemperance as a leading cause of disease. We may safely infer, therefore, that what was not lashed as a prevailing vice by the satirist, or noticed as a habit prejudicial to health by the physician, and which could not have escaped both if it had existed in any prominent degree, must have been really of very rare occurrence, even if we were not in possession of more direct testimony to a similar purport.

"We must therefore be content to claim for the habits of intemperance which are now so prevalent amongst us, a less illustrious origin than from among those ancient leaders in the arts of peace and of war, many of the works of whose purer times and better genius have descended to us, as worthier memorials, still unrivalled in that excellence which has conferred their title to immortality. It is even deserving of remark, that those nations which have the best claim to be considered as their direct descendants, have inherited from them, in a large proportion, this virtue of temperance, if in many other respects they may be held to have degenerated. And yet, even in Italy itself, there appears reason to fear that this commendation may become less and less merited: several eminent Italian authors speaking of habits of intemperance as a rapidly increasing evil. In the great, and still, in many respects, unrivalled work of Morgagni, cases, displaying the deplorable effects of intemperance, are strewed everywhere among the results of his experience; while in recent times Giacomini complains, in strong and feeling language, of the habit as continually extending more and more widely among the populace, so that the memories of all medical observers were full of examples of its ruinous effects. And Speranza, besides stating his own experience of the large proportion of diseases of the digestive organs, arising from the use of spirituous liquors, quotes also that of Perrone, of Naples, as to the frequent occurrence of fatal maladies, the fruits of drunkenness, among the lower classes of that city. But it is doubtless to the great Teutonic family, from which we derive the larger portion of our mixed descent, that we owe the unhappy inheritance of our propensity to the use of intoxicating liquors. With the primitive Germans, as described by Tacitus, in traits which are everywhere revived to us in the later, though still remote traditions of that hardy people, it was considered no reproach to pass night and day in continuous drinking; and these revels, he tells us, were rarely terminated without strife or bloodshed. It appears that the temperate Romans knew how to bring this propensity into subserviency to their ambitious aims; for the historian adds, that if the barbarians were indulged in their drunkenness by placing the materials, usually a kind of ale prepared from grain, within their reach, it was not less easy to conquer them by their vices than by the sword."

It is among the northern and ruder nations of Europe that drinking has always prevailed to the greatest excess; and hence it has been inferred that the vice may be attributed to severity of climate; nor do we think this can be altogether denied. True it is, the descendants of northern tribes who have come to occupy milder latitudes retain the tendency of their ancestors. But a habit once established does not necessarily cease, but may be continued, and even extended in its operation, though the cause which produced it no longer exists; and in all reasonings on these and kindred subjects, it is a very just and philosophical remark of Dr Wilson's, that it is always true in nature, and therefore true in science, "that neither natural phenomenon nor natural effect has ever been found to depend for its being on a single cause, or a single condition." What we would contend for is, that, while not denying the influence of race, to which there seems at present a great tendency to refer at least as much as is due, we should not forget to consider as well the effect of climate and circumstances in modifying the character of men, and influencing their habits.

The next chapter is on conviviality, the first step in the progress, first, to casual, and, finally, to habitual intoxication, which form the subject of the next three subdivisions. On these we cannot pause to comment; they are all so good, that we should have great difficulty in selecting, and therefore can only recommend them to careful perusal. After having discussed the phenomena of inebriety in all its stages and degrees, the author next proceeds to consider, first, the constitutional, and then the mental effects of drunkenness. A section on suicide follows. He then gives us its Physiological Pathology, and concludes with a chapter on prevention and cure.

There is no doubt that alcohol is a poison and one of great power. Taken undiluted and in any considerable quantity death rapidly, and if the dose be very immoderate almost immediately, follows. Though the instances of this kind of poisoning are not very numerous, they are amply sufficient to establish the fact. When death is thus instantaneously produced, the brain and lungs are found deeply congested, and the blood dark coloured, and Dr Wilson justly states that this form of death which partakes of the nature of asphyxia, may more probably, sudden as it is, be ascribed to the absorption of the poison, than to any supposed action it exerts on the nerves of the stomach. The phenomena of poisoning have been carefully and successfully examined in late years by many distinguished investigators. The theory of absorption is just now in the ascendant, and these researches have partly contributed to the revival of that modified numeralism, which now prevails in medical theories. Though it is travelling out of the record, we must remind our younger readers that these views are after all rather a revival in a modified shape, and adapted certainly to a much more refined and subtle chemistry than that which prevailed in the days of Boerhaave, of opinions that prevailed universally, less than a century ago; and when we think that theories, whose apparent demolition delighted our own fathers, are now resuscitated equally to the delight of their grandsons, there seems some appearance of reason in the opinion of those who assert that the progress of medicine is one of rotation rather than of advancement. At all events we would like if it were clearly pointed out, and we know few men better qualified for the task than our author, how much of our present views is new, and how much old, and, still more, how much is sound and well established, and how much must still be regarded as merely hypothetical. Such an enquiry, conducted as Dr Wilson is evidently well qualified to conduct it, would tend to keep certain medical writers in mind of what they are sometimes apt to forget, that medicine is a practical art, as well as a branch of natural science, and that the chief value that inquiries in its various subdivisions possess, must be derived from the possibility of making their results subservient to the great object of the art, the preservation of health, the removal of disease, and the alleviation of suffering.

But even when alcohol is not absorbed in such quantity as to prove rapidly fatal, it is still absorbed, and its inevitable *tendency* is to produce, though more slowly, deleterious effects on the animal economy, and *that* over and above the inflammatory action, more or less acute, which it excites in the stomach itself. The same congested or inflammatory state of the bloodvessels, which alcohol produces in the stomach, is speedily established in the liver; also by the transmission through the vital circle of bloodvessels of blood of a vitiated quality, and loaded with impurities which that organ is unable to eliminate. Structural change supervenes upon excited or perverted action; the nature of the changes Dr Wilson minutely describes, apparently considering them all modifications of fatty degeneration, from the excessive quantity of carbon contained in the blood, partly from the presence of the same element which originally produced the changes, partly from the deteriorated secretory power of the liver, which throws still other excrementitious matters into the blood, which goes forward from it to the heart. Sometimes simultaneously, sometimes consequently, all the other organs of the frame are liable to suffer in their action, and in their texture, the heart, the lungs, the nervous centres are all implicated more or less, the extent to which they may become diseased, or the rapidity with which they are involved depending, in great measure, on the predisposition of the individual, or the specialties of the case. The chapter on physiological pathology, of which we have thus briefly indicated the character rather than analysed it, may be said to form the *rationale* of those on special results which precede it. We are not sure but it might have been better, although it might have endangered a little repetition, if there had been a little of the physiology mixed up with these chapters. No person who reads the book attentively can fail to see the close connection between the cause and the effects, but we are inclined to think the *modus operandi* might have been brought to bear more impressively on the popular mind if they had been placed a little more in juxtaposition. We would willingly quote liberally from this chapter, were we not afraid of extending this paper too far, but we cannot forbear presenting to our readers Dr Wilson's view of what has been called spontaneous combustion, as a specimen exhibiting rather a difficult subject, discussed in a very able and philosophical manner, and illustrating Dr Wilson's mastery of principles, and the acute, and at the same time dispassionate way in which he deals, both with evidence and the opinions of other men. At page 99, we have a careful and detailed analysis of the recorded cases of this frightful occurrence, and at the close of the physiological chapter, we have the following explanation of the phenomenon—

> Lastly, it now seems comparatively easy to offer a rational explanation of that condition of combustibility of the body, which is fitted to give rise to the description of burning usually designated spontaneous combustion. Few now believe in the strict accuracy of the latter designation, or in the doctrine which it involves, and fewer still will credit it after reading the clear, though, it must be confessed, somewhat dogmatical exposition of Liebig. But when this distinguished chemist proceeds so far as to question, whether the ordinary degree of combustibility of the human body be itself really capable of increase in any remarkable degree, he seems to fall into that logical error, of inferring without weighing all the possible circumstances, which he rebukes so sharply in others; and leaves us to desire that an equal familiarity with the pathological room and the hospital, as with the laboratory, had placed him in a better position for judging of the manner in which the action of external agents upon the living structure may be controlled by circumstances, or modified by disease. If

it have been proved that the blood has carried with it the alcoholic fluid to every portion of the body, imbuing the inmost recesses of structure as it traverses the more minute ramifications of the bloodvessels; if it bear with it thither also a considerable portion of fatty or oily matter in all parts of its circuit; and if, moreover, it has everywhere laid down this fatty substance in remarkable exuberance, either, as a simple deposit, or, what may be here of far greater importance, as an actual conversion and degradation of other structures into its lower form of animality; it is manifest that we have here abundant materials of an inflammable nature, which must present a ready fuel to any casual application of fire which may reach them from without.

The objection that a non-combustible substance, or one difficultly combustible, is not rendered more so by being joined to a more combustible one, falls to the ground, when we thus remember that, with the advanced drunkard, it may be less a combination of a substance of slow combustion, like flesh, with one of ready combustion, like fat, than an absolute conversion of the former into the latter; and therefore the existence of a combustible substance more exclusively. And even this fat of the drunkard differs in its characters and aspect from ordinary fat. May it not have acquired properties which render it still more readily combustible? What new combinations of alcohol, or its constituents, with the organic substances, dependent on what molecular or isomeric changes, may be effected by the chemistry of the living system under such circumstances, has assuredly never hitherto been ascertained so fully as to entitle even the most ingenious speculator to speak dogmatically, when he has to oppose facts which have not yet been controverted, or proved to be capable of any more certain explanation. Professor Bischoff, like Liebig, insists strongly on the large proportion of water in the human body, as inconsistent with the possibility of its entering into combustion. But water may be expelled by heat, from the combustion of other materials, existing in more than ordinary abundance. Neither does this objection come reasonably from a chemist; for if water itself does not burn, its elements do, and chemistry and electricity easily produce the wonder. Electricity separates the combustible material, and applies the spark; and potassium starts into flame upon the moist surface that supplies the fuel. Such facts, alone, place us in a position which, if it does not enable us to explain the phenomenon of spontaneous combustion, in the sense in which many have understood it, precludes us at least from denying peremptorily the possibility of its occurrence; if we are to be urged to reject the authority of evidence, and to take the unstable ground of relying exclusively upon *a priori* reasoning. Besides, it is unjust to argue unhesitatingly from facts in ordinary chemistry, to processes in the living organization. Professor Bischoff appears to question this, and asserts that, if it be true that 2 + 2 be 4 in chemistry, it must be true everywhere else. But we must first well assure ourselves, that what we know to be 2 in chemistry, and what we assume to be 2 in organic life, are really identical quantities. Failing in this, we fail utterly, and have been merely wasting ingenuity upon an abstraction. And let it be remembered also, that the sufferers have been nearly universally females, in whom the carbonaceous, or combustible principles naturally predominate, as the nitrogenous in the male. It would certainly savour somewhat of simplicity, to quote Plutarch as an adequate authority, at the present day, on any point of scientific speculation; but where he speaks to matters which, from the customs of the times, naturally came under his own observation, to an extent which cannot occur now, his known honesty renders him an available witness. At all events, it appears interesting to notice, that he mentions it as a usual practice of the officials, in burning the bodies of the dead, to "put one female body with ten males upon the same pile;" for that, he adds, "contains some inflammable and oily parts, and serves as fuel for the rest."

With this concurrence of circumstances, then, and thus ripened for the consummation, the unhappy drunkard sinks down in the stupor of intoxication, and comes into contact with a burning candle or with glowing embers. The fire penetrates the surface, and approaches the more combustible matters beneath. The fat melts, and the mingled oil and spirit burn with a smouldering flame, which is kept subdued by the admixture of other animal substances less inflammable, and yet attains sufficient power to consume the solid portions of the body, themselves, even to the bones, deeply interpenetrated with fat. Doubtless all the requisite conditions must concur, in their fullest development, to admit the production of this extraordinary effect. It can be no argument against the explanation, that the body of a healthy person, poisoned by alcohol, is not susceptible of this form of ignition; because the very fact of the previous healthy condition places the individual beyond the limit of efficient causation. Any one may ascertain, however, that a portion of fat, which has been steeped for some time in ordinary proof spirit, may be made to burn without difficulty, with the aid of little extraneous combustible matter; and that the flame actually presents the dull lurid appearance which has been described as characteristic of the combustion of the drunkard. But let the explanation be what it may, the fact of the occurrence appears as strongly vouched as it is deeply impressive: and, if it constitute a strange and terrible visitation, it at least depends upon too large an amount of evidence to be hastily rejected as incredible.

We have now arrived at the last chapter of the book, which is devoted to the consideration of the prevention and cure of drunkenness. As the disposition to indulge to excess in alcoholic drinks depends in the individual on that infirmity of moral resolution which induces a man to prefer a present gratification to a future good, the great means of cure must be sought in the higher moral training of the people,—in the establishment of a high and rational scheme of education, teaching men to trace consequences, and calculate tendencies, and to regulate their conduct by these, instead of headlong impulse,—in the diffusion of sound religious principle, not merely as a system of dogmatic instruction, but as the basis of rational conduct in all the relations of life, a point of view in which we must say religion, among our multitudinous sects, is far too little considered. Training of this sort, as Dr Wilson remarks, to be effective, must begin early, and be continued long, and its application to personal conduct, perhaps, in the way that Dr Wilson suggests, of making a simple code of ethics a portion of school education (at all events in some way), made clear and unmistakeable. Everything too that tends to foster habits of providence and to stimulate the hopes of working-men should be encouraged, savings banks should be established, and partnerships with limited liability, which we believe would greatly excite the operatives to try to accumulate, should be allowed. Regarding the amusements to supply the void that must be left if the use of stimulating drinks were abolished, we should have liked Dr Wilson's opinions at greater length, chiefly because it would have led him to discuss the dislike or convivial use of various beverages, and the drinking usages of society; subjects to which we would respectfully invite his attention, when his work reaches, as we hope it soon will, a second edition, and on which there are some valuable remarks in that very interesting, delightful, and highly suggestive book, Hugh Miller's Schools and Schoolmasters.

With regard to the two most important measures of what may be called a special character, in reference to the prevention of drunkenness, temperance societies and legislative interference, Dr Wilson writes with that philosophical moderation and sobriety which we think one of his most admirable characteristics, though with an evident leaning in favour of both, especially the latter, in which we hope the public mind is being gradually trained to agree with him. It is an undoubted fact, that all nations and races of men have sought for and devised some form of inebriating liquors, and employed them on occasions of social indulgence; and it is a question worthy the consideration of all enlightened advocates of temperance, whether the cause is most likely to be surely promoted, by seeking altogether to eradicate what seems a natural craving, or by bringing it under moral control? Regarding the enactment of laws for the promotion of temperance in the prevention of inebriety, it is clear that much discussion is needed before the public mind is ready to determine on any specific measure; but undoubtedly a certain progress has been made—the *laisser faire* principle has been abandoned in many cases, where the health and morals of the people are concerned, by our most enlightened politicians, and we venture to predict we shall hear less and less of that arrogant talk, so omnipotent a few years since, which consisted in a misapplication of the maxims of political economy. The example of the Maine Liquor Law, too, cannot be lost sight of by British philanthropists and legislators. We by no means recommend the adoption of any particular measure; the public mind is not ripe for it. It may be said, that so far as the popular mind is concerned, it is still in a state of *incubation,* and till men's views are more matured, it would be premature for any legislature to commit itself, for a law can only be useful when it reflects the moral sentiments of the community. Then, and then only, does it react on the feelings from which it originated, and in so doing, secure the means of its own permanence and efficiency. To guide and develop these sentiments and feelings no book can be better adapted than the little work which we now take leave of. It is the work of a thoroughly and highly educated man, of a man of large experience, great attainments, and of a cultivated and powerful understanding; and one whose regard for truth never permits him to overstep the modesty of nature for the sake of heightening a description or urging an argument. It is judiciously timed and equally excellent in execution, and admirable in intention.

The Use of the Blowpipe in the Qualitative and Quantitative Examination of Minerals, Ores, Furnace Products, and other Metallic Combinations. By Professor PLATTNER, and Dr SHERIDAN MUSPRATT, Founder and Principal of the Royal College of Chemistry, Liverpool. London: Churchill.

This is a work consisting of about four hundred octavo pages, that ought, if it had been written at all, to have formed a chapter, and that not a very long one in some system of mineralogy. It professes to be written for the use of miners who have no access, or but an occasional one, to a chemical laboratory; and its affected aim is to instruct such individuals in the manner of discriminating various minerals by means of the blowpipe. Nevertheless, the book is full of the very pedantry of chemistry and mineralogy, and the style is the most inexcusable compound of bathos and bad English that it has ever been our fortune to meet with in a work professing to be a scientific one. We have only space for one extract. It is taken at random from an article on copper, which, in so far as respects the miner who has no means of going to a laboratory, might certainly be compressed into half a page. The author or authors are speaking of the forms in which copper occurs in nature:—

"5. With chromic acid and chromate of lead in *Vauquelinite.*

"6. With silicic acid and water in *Dioptase* ($3\ Cu\ O\ 2\ Si\ O^2 +$ 3 aq.), with silicic acid, water, and a little carbonic acid in *Chresocella,* and with silicic acid and silicate of alumina in *Allophane.*

"Further, copper, is not only found *per se* in scoria, *et cetera,* produced in the smelting of cupreous ores, but very often as a secondary constituent in the slags from argentiferous and plombiferous metals. It is found,

"*a.* Metallic in pure *Gaarkupfer,* the product of the third melting of the cupriferous ores, and in combination with other metals in *Schwartz kupler,* second product; in the *Frischstucken*—the argentiferous leads, obtained in the refining of copper ores for silver; in the *Sargerdornion*—the residues which remain after extracting the lead from *the Darrlinge,* and which yield a crude copper on being subjected to a process of smelting; in the Darrlinge—eliquated coppers, from which the silver has been sweated out by lead, and in *Cupriferous Workable Lead;*

"*b.* With sulphur, *per se,* and also with sulphides, *exempli gratia,* in *Rohstein, Blerstein, Kupperstein, Kupfenteg*—one of the products produced by smelting the roasted *stein,* which is obtained by smelting roasted *Blerstein* with quartzose copper ores, with Blestein slags, and quartz and in different *Tullys.*" (P. 192.)

Every page of the work is filled with matter equally well and clearly expressed, and quite as much calculated to be of service to the miner. The book is simply an unscientific one, and would scarcely have received even this notice were it not for another reason.

It has, as the heading of this article will indicate, two authors—Professor Plattner, and Dr Muspratt, Founder and Principal of the Royal College of Chemistry, Liverpool. The latter gentleman adds that he is the author of "The Chemistry of Vegetation." This last assertion he will, upon reflection, surely somewhat modify, and therefore it may be passed over. For anything we know to the contrary, Professor Plattner has held a chair in some regularly-constituted university, and is really a professor; but as the name of his university is omitted, ignorant people may suspect him to belong to the class of professors of which Professor Anderson, the Wizard of the North, is the distinguished head. In the advertisement of the work, too, we notice that its English editor (who takes liberties with his mother tongue) is also styled Professor Muspratt. We were not previously aware of the existence of a university in Liverpool.

We feel bound to add that the statements made regarding the behaviour of minerals before the blowpipe seem accurate enough.

PART III.

PROGRESS OF MEDICAL SCIENCE.

FORENSIC MEDICINE.

Trial of George Lillie Smith, M.D., and Robert Campbell, before the High Court of Justiciary for Wilful Fire-Raising, 15th January 1855.

Counsel for the Crown, Lord Advocate Moncreiff, and Mr D. Mackenzie, A.D.; for Dr Smith, the Dean of Faculty, Mr D. Mure, and Mr John Millar; for Campbell, Mr G. Patton and Mr A. B. Shand.

The prisoners were charged with having on the 30th September, or the 1st October, in or near the stack-yard of the farm of Haughs of Kinnaird, parish of Farnell, Forfarshire, occupied by Mr John Smith, wilfully set fire to one or more stacks of grain, by applying to them lighted matches or other ignited substance, the fire thus wilfully applied having taken effect and burned the whole stack-yard, containing 55 stacks or huts of grain. The prisoner Smith is a man of florid aspect and middle age. The prisoner Campbell is an old man of simple appearance.

Both pleaded not guilty. A special defence was put in for Smith, denying the libel, and averring that when the act was committed, he was insane; for Campbell, denying the libel.

The first witness was Jane or Jean Mackenzie. She was servant last September to Mr Smith, Haughs of Kinnaird. Went to Brechin Saturday 30th September, and left to return between 10 and 11 at night. When she came near the Haughs, observed light in the direction of Haughs; this was about a quarter before one in the morning. She went up to Haughs, and observed the corn-yard on fire; 3 stacks next the corner, *i.e.*, the north-west corner. She gave alarm to the other servants, and to Mr Smith. All the stacks were burned, but not the steading; but cannot say how long it burned. She did not recollect in what direction the wind was blowing. It appeared the corn-yard had been set on fire, and the steading would have also been burned, if the wind had not turned, as it did, in the course of the burning.

John Smith, tenant of Haughs of Kinnaird. He had known, for a considerable time, Dr Smith, who was medical attendant to his family. Witness had a difference with his wife last year, and in consequence, she lived in Edinburgh some months, and returned to his house in September last. On the 30th September 1854, being Saturday night, he (Mr Smith) saw the steading all right about eight o'clock at night; went to bed a little after nine o'clock; all right then as far as he was aware. He was awoke a little after one in the morning, and found the stack-yard on fire. He found a number of the servants about the place, but is not aware that they had done anything to stop the fire before he came. He did all he could to get it stopped. All the stacks were burned; 51 stacks and 4 small huts. He should say two-thirds were on fire, when first he saw them. All the stacks and huts mentioned in the indictment were burned. He sent a servant to Brechin for the fire-engines. Brechin is about four miles from Haughs by the road they call the Brechin road. The steading is about two miles from Bridge of Dunn, which is to the east by north. Engines came and assisted putting out flames, by which about half a stack was saved though damaged. Value of stack-yard at that time about £2000; would have been now worth considerably more from rise in price and lowness of valuation then put on it. There was little wind, but what there was was blowing from the north-west, in the direction of the house; thought it changed slightly after all the stacks had taken fire. The steading is betwixt the house and the stack-yard. The house is between nine and ten yards from the steading, and the steading about three yards from the nearest stack. Witness suspected Dr Smith immediately, because he had sent his wife a threatening letter some days previously. Shown a letter dated Montrose, September 25th.

The letter was read by the Clerk as follows:—

Montrose, September 25th.

MY DEAR MRS SMITH,—I still call you so for all the iniquity you have done me. I leave to yourself to think "I'll keep all my promises to you. I'll do nothing without telling you." As to your returning to Mr Smith, I should have been as happy to give you my arm to go back, as I was to lead you away, had you told me. You have much to think of!

All I ask of you is an interview with me or write me. My intentions at present are determined thereby. You know my proposed movements, but they shall be delayed according to circumstances.

I have been ready to help you in trouble, but be assured, I am one as ready to act when I am opposed. I am not one that will be shuffled.

I ask of you to write me first, and then to give me an audience. If you do not do so, I shall have one perhaps less agreeable to you. As your friend as I have ever been, I ask you to do so. Blame me not, but my blood is boiling, and retribution I shall have in one shape or another. I care not though this comes to his honour's hands, you may show it him. Your happiness *depends on your answer to me.* I am not in trim to be trifled with or *get duped.*

For all you have done against me and insult offered me, I say—My dear Mrs Smith, ever your sincere friend,

(Signed) GEORGE L. SMITH.

P.S.—Give me an answer or you shall everlastingly regret it. G. L. S.

I confess I walk out of the course of a gentleman in writing you this, but it is only my regard for you that has caused me to do so. Recollect for once and all, I am one *who will act as I say.*

(Signed) G. L. S.

His wife had shown him that letter the day after she got it (26th September). It had been brought by the girl Clark, daughter of Dr Smith's housekeeper. Dr Smith had a residence in Montrose, but had been in Edinburgh most of the time witness's wife was there; during greater part of that year witness had not spoken to him; he meant he was not then on speaking terms with him. Witness saw the girl come to the door; he answered it; the girl asked for his wife, who went to her. Witness did not see Dr Smith's letter delivered. Identified Dr Smith's handwriting. Shown a stick which he identified; he got it in the course of the following week from Robert Gordon, a blacksmith. From the letter, believed Dr Smith was ill-pleased at Mrs Smith's returning to her husband's house. The Esk runs to the north of witness' farm, and there are several fields between them. It was very dry on the night of the fire, and had been long so. The soil near the river is generally gravelly and not red. The policeman came about two o'clock in the morning, and witness told him whom he suspected. There is no red clay land on witness' farm, but when earth has dried on clothes it has a red appearance. It is not like moor-land.

Cross-examined by the Dean.—Various soils on the farm—clay, also loam mixed with sand; no other. Had been living in a state of separation from his wife previous to the fire. She returned on 21st September, they separated by agreement, and she had been living in Edinburgh. Witness had been acquainted with Dr Smith ever since he came to Montrose; think 17 or 18 years ago. Not intimate during that time, but from autumn of 1852, on intimate terms, till they differed about 14th January 1854. Q. During the year 1853 was there anything remarkable about his appearance, manner, or conduct? A. Yes, I should say there was in all three. He complained of being unwell, and often appeared excited, occasionally very much excited. At first I thought it proceeded from illness and from the state of his affairs, but latterly I thought he was taking too much drink : latterly I ascribed excitement to taking too much drink. Q. Do you still ascribe it to that? A. More so than ever. Q. Have you met with him since disagreement. A.—Met him in brother-in-law's house in the course of the week after our difference, saw him occasionally since that. Conversed with him 12 or 14 days after that, about the end of January 1854; not since.

Cross-examined by Mr Patton for Campbell.—Witness had seen Campbell once or twice before; had no other knowledge of him. Never had spoken to him—saw him once in Dr Smith's house.

Re-examined by Lord Advocate.—What is your reason for ascribing it to drink more than ever? A.—Because since witness paid his accounts he finds that a great deal more drink was used, in his house, when Dr Smith was there, than ever before. He was much there in 1853, and much more drink was then used than I knew of at the time.

Examined by Lord Justice-Clerk.—Dr Smith had opportunities of getting that drink without witness's knowledge. He found fault with witness's wife for having locked the sideboard, and it was left open. He often slept in the house, and witness knew he was in the habit of having wine placed in his bed-room. Witness did not know of his own knowledge that Dr Smith took drink from the sideboard.

Witness was asked to describe the excitement. A.—Dr Smith swore and used very violent language. Would not say that it was altogether the effect of drink. He said he was unwell, but witness began to suspect he was taking more drink than was good for him, and that this was partly the cause of his excitement. Witness told him so. Witness had always acted to Dr Smith with great kindness, more than he ever did to a brother. Except from himself, he had no reason to know he was unwell. A medical man attended him occasionally. He should say drink would account for all the appearances of excitement he saw.

By the Court, on the suggestion of the Dean.—He was attended by medical men, in witness's house, from Sunday, December 4th 1853, for about a fortnight—by Dr Booth, a medical man. Witness had insisted on this doctor, or some other, being with Dr Smith constantly in the house till he got better. Because afraid to leave him without some one to look after him, while witness was absent. He was so much excited; was drinking so much at that time. Witness saw him drinking large quantities of wine and brandy during that fortnight. Saw him take several glasses of brandy at a time during that period. Dr Booth and Mrs Smith would see this too. Witness insisted on a nurse coming with Dr Booth and attending, and Mrs Clark came. She was there all the time. Witness at the same time told Mr Smith's partner in business, Dr Officer, he should see him every day.

Q. *By Lord Justice-Clerk.*—He (Dr Smith) was much more excited on this occasion than formerly. About the end of the first week of this period, he struck Dr Booth and witness's wife. Witness understood it was from drink, which he had got from the nurse, Mrs Clark. Witness told his wife, unless this was discontinued, he would take the keys of the house from her and keep them. Prior to the 4th December 1853, Dr Smith was at Montrose. On Saturday, the 3d December 1853, witness returned home, from a market, and found his wife from home, although he had left her in bed unwell; and was told that Dr Smith had sent a message, wishing her and witness to go to see him, as he thought himself dying. Witness went on Sunday before church to see him, and he pleaded hard to be allowed to go to the Haughs, and he (Dr Smith) would sleep in any of the outhouses or farm-houses. Witness then consented to his going, on condition of his taking a nurse with him, as he would not on any account permit his wife to attend him. He was no relation of the witness. His partner and the nurse came with him on Sunday, and on Tuesday he found him so excited, that he sent for Dr Booth. His partner had been sent for on Monday night, in consequence of his being so excited, and on Tuesday witness sent for Dr Booth. Witness's house is 6 miles from Montrose. On occasion when he drank the several glasses of brandy, which was on the Friday following, he, in the course of the evening (7 or 8 o'clock) was walking about the dining-room in his night-shirt, in so excited a state, that they were glad to allow him to take the brandy to get him to bed again. He first asked for wine; the medical attendant wished to confine him to six glasses of wine per day, but this had not been done. They had given him wine and a glass or two of brandy, and he helped himself to more. Dr Booth had apparently no authority over him, at that time, as the nurse had been giving him drink unknown to Dr Booth and Mrs Smith. On the Friday evening, after he got the brandy, they got him prevailed on to go to bed. He was then raving from drink. Witness does not pretend to say his excitement was all from drink, but his opinion is that it was so. On the Saturday (next day) he got more excited, and witness then insisted on his being confined to a certain allowance. He then got, witness understands, about 1½ bottles of wine per day, and he became rapidly better, and from being unable to rise, by Friday following he was walking about the steading shooting pigeons with Dr Booth.

Q. Did he dwell on any particular subject during this excitement? A. He generally dwelt on differences he had with his wife. He was married and was living separate from her. Sometimes he was angry at what he said she had done, and melancholy from the effects he said it had upon himself. He had been separated from his wife, witness thinks, from the beginning of September 1853. She had been away and taken back again. Not aware whether before that time he was given to drink much or not. Had occasionally met him at dinner parties, but otherwise not much in his company, previous to September 1853. Not much in his company between September and December 1853. Witness avoided asking him, during that period, to remain all night, in the house when he came. Not a sudden change, he saw it gradually from May 1853. Habits had then changed. It was then he first told them of differences with his wife. After that he was getting worse. Would occasionally come to witness's house perfectly well and behave like a gentleman. About the middle of January 1854, he left the house on a Saturday, and this, witness may say, was the commencement of the quarrel between them. He saw nothing of his drinking after that. He saw him once or twice in Edinburgh, on the street, when he was living there. He saw him after he returned, prowling about his (witness') steading, on 30th August. This was before witness's wife returned; he did not speak to him, but went into the house to avoid him.

Margaret Smith, wife of last witness, had a dispute with her husband last year, and went to reside in Edinburgh. Occasionally saw Dr Smith there, and took his advice as a friend. Returned to her husband, September 1854. Dr Smith was not aware she was to return. On Tuesday, September 26th, after she returned, got a letter from Dr Smith. Identified the letter before referred to. The letter was brought by the little girl Clark. She (witness) gave it to her husband, and sent no answer. Got another letter on following Saturday. This was the night before the fire, she thinks. Fire on Sunday morning. Witness tore this last-mentioned letter, but some of the pieces were preserved. Being shown fragments of the letter libelled on, identified them.

Q. What was the general purport of the letter? So far as witness read it, it bore that Dr Smith had been at Noranside to see Miss Carnegie, who had asked him to write the letter. Witness answered neither of the letters. She was awoke on Sunday morning by an alarm of fire. The fire consumed the stack-yard, but did not reach the house or steading.

Cross-examined by the Dean.—Witness' acquaintance with Dr Smith began six and a half years ago; after her marriage. Q.—Did you observe any change come over his manner? A.—He had always been the same ever since she knew him.

Re-examined.—Dr Smith resided in their house for some time in December 1853, with doctors attending him and a nurse. His head was very much affected; he had great pain in his head, at times he was very much excited. Q. Did he drink to excess while living with you? A. He drank a quantity of wine, but always got very sick after it, and it left his stomach very quickly. It was prescribed to him. He did not drink spirits at that time, he was allowed brandy, he took what was allowed him; he had two glasses of brandy a day allowed him. Cannot say whether he took more or not. One day in particular, he was in a more excited state than she had known him before. She is not aware of any apparent cause for this excitement on the one day; he complained of his health when witness saw him in Edinburgh. He was not constantly excited. At times he was, at times he was not. She did not meet him to converse with him after she returned from Edinburgh. He was liable to these fits of excitement all the six years she knew him, but she saw little of him till within the last three years and a half, and it was then she observed the fits of excitement.

Re-examined by Lord Advocate—He intended to go abroad; talked of this to witness when she was in Edinburgh, and that he intended to apply to Government for an appointment. Does not know if he did or not.

Alexander M. Officer, M.D., partner of Dr Smith since July 1853.—He has known Dr Smith for six years; he is aware that Dr Smith was intimate with Mr and Mrs Smith at Haughs of Kinnaird. Dr Smith lived some time at Laverock Bank, near Edinburgh, while Mrs Smith was in Edinburgh. He returned to Montrose about 8 or 9 days before the fire. He seemed very dull; cannot say the cause; but he said is not this a pretty business of Mrs Smith returning? said he was disappointed at it, and would not have expected it, as she had said she would not. On Monday evening he read to me a letter he had written, and said he was to send it. Witness advised him not to send it. He would not take witness's advice. He told me he had been at Haughs of Kinnaird the day before, *i.e.* on Sunday. He did not state his object in going. He expressed himself violently in regard to Mr Smith, who, he said, had attempted to injure him by imputations on his character, and that he wished two questions answered, but did not say what the questions were. Said he was determined to have these questions answered. Witness is aware that he was applying for a situation as an emigrant surgeon. He wanted these two questions answered before he went, and used threats of revenge if these questions were not answered, —threats of a general nature, not specific. On the Saturday he read to witness another letter which he meant to send to Mrs Smith. Witness last saw him this Saturday afternoon, between 5 and 6 o'clock. Mr Somerville, Mrs Clark, and one of his children, were with him. He said he was going to Noranside, where Miss Carnegie then lived, to let her know regarding his voyage; he was to go in Mr Somerville's gig, and he left with him about 6 o'clock and said he would return to Montrose the next day. Witness heard next morning of the fire at Haughs of Kinnaird. Heard this from Mrs Clark, Dr Smith's housekeeper, between 8 and 9 in the morning. Afterwards heard it from Mr Driver, a farmer in the neighbourhood, who said Dr Smith was suspected. Witness called on Mr Smart, Montrose, an acquaintance of Dr Smith; and in consequence, went with Mr Smart from Montrose to Noranside, direct by Brechin; saw Dr Smith at Noranside, along with Miss Carnegie, and Dr Steel of Forfar. Mr Smart told Dr Smith of suspicion being against him. Dr Smith asked if the houses were burned? Mr Smart said no. He then asked if Mr Smith was burned? Mr Smart said no. Dr Smith said perhaps it is as well, as burning would be too good a death for him; he did not say whether he had done it or not. He (Dr Smith) when he was told he was suspected, did not say he had *not* done it. Witness in the afternoon was alone with Dr Smith at Noranside; conversed with him on the subject. He did not tell him he had been at the Haughs that night. He said he had been there, but he fixed no date. Witness's impression was he meant the previous night. Witness advised with others there, that Dr Smith was in an unfit state to be at large,—witness meant with Dr Steel, Miss Carnegie, and Mr Smart. He gave no especial advice to Dr Smith—he said to him, that even if innocent he would be apprehended on suspicion, which would be injurious for him, and he had better go away, as he was to take a voyage, at any rate. Ultimately it was settled he should go to some asylum; when proposed he should be taken under restraint, this was overruled, and he was advised, but not in my presence, to go on a visit to Dr Brown at Dumfries. Dr Steel and Dr Smith left together, and witness left almost immediately after, taking Mr Somerville's gig and horse with him, which Dr Smith had brought to Noranside. When witness got to Montrose, he gave directions about Dr Smith's clothes to the girl Clark, that her name should be put on the things, that they should be addressed to Coupar-Angus station; that she should go there with them, and that Dr Smith would meet her there. In the course of next afternoon witness heard that Dr Smith was apprehended. He did drink considerably latterly. He first mentioned to witness he was about to go abroad, about 18 months ago. Then, what he proposed was only a long sea-voyage for his health. Witness recollects his receiving the letter, now shewn, from the Colonial Land office, 27th September 1854. Printed testimonials for him were packed up with the things witness sent to Coupar-Angus for him, also his diploma. All the testimonials were of old date. Dr Smith said he had referred to Taylor of Belfast, Mr Smart of Montrose, and other gentlemen.

Cross-examined by the Dean.—Got to Noranside about one o'clock, and remained there till about a quarter past nine in the evening. Found Dr Smith in the drawing-room with Miss Carnegie and Dr Steel. There was a good deal of conversation with them about him. Witness had the opinion at that time as to the state of his mind, *i.e.*, that he was insane. Witness had been acquainted with him about six years. Had treated him for congestion of the brain, and disease of the liver. The disease of the liver was very severe. Had not altogether produced the congestion of the brain, but it contributed to it and aggravated it. He was treated also by Professor Christison, Drs Steel, and Booth. He had this complaint since the spring of 1853 to my knowledge. It is the nature of disease of the liver when so enlarged as his was, to grow worse. There was at one time appearance of paralysis, about December 1853 or January 1854, from the state of the brain. This was when he was living at Haughs of Kinnaird. Witness visited him there, also Drs Booth and Steel. He had a severe attack then. Professor Christison visited him in September 1853, when they came to one mind about it, that there was no hope of his recovery. The nature of the complaint was what witness has stated. No congestion of brain then, chiefly disease of liver. In December there was congestion of the brain; both legs were afterwards a little affected, but more distinctly the right leg. From what witness saw in September 1853 he expected his case to result in insanity. Still more strongly of that opinion in December 1853. Saw him often; frequently in state of excitement, sometimes very dull, at other times very high. On the day spoken to at Noranside, his eye was restless; seemed calm in his movements when they went in. Witness don't recollect who first suggested that morning that he should be sent to an asylum. All present were of the same opinion. When he told witness he was going to Noranside he said it was to tell Miss Carnegie he had got the appointment to the ship. She was a friend of his. Witness consulted with Miss Carnegie and Dr Steel outwith Dr Smith's presence, and resolved he should be sent to an asylum. Witness did not communicate this to him. Dr Brown, at Dumfries, was an old friend of his. Witness had a talk with Dr Smith at Noranside that day by himself, but not after this. He said he had been at the Haughs; had had Mrs Smith in his power; had been in their bed-room; he was about to shoot both Mr and Mrs Smith, and then thought it would be too bad to shoot two parties asleep. At the same time he said, as he was going to shoot Smith, his wife was between, and he was afraid he would shoot her in place of him. Fixed no date for this, but witness understood it to be that night. He had no doubt it was a delusion, as he thought he could not have been in the room without their knowing it. Has no doubt this was an insane delusion, produced by disease, and having a special reference to the Smiths. But it was not a delusion that he had been at the Haughs. He was much excited about them previously. He took Mrs Smith's part in her differences with her husband. There was nothing in these differences that would have excited a sane man in the way he was excited. Witness is not aware of anything very peculiar in these domestic stories. Some days before the fire witness had conversed with Dr Smith, when he said he had four men coming from Perth, and asked witness if he would go to Haughs with him; said the men were Highlanders, with dark lanterns, and that he wished to get other two, who might surround the Haughs' house, and he would then break open the door and go up to the bed-room, and demand an explanation of these questions, *i.e.*, the questions referred to in his letter, but which he did not explain to the witness. If not immediately answered he would say to each, there's a pellet for you, and shoot them. This was to be in the night. Witness does not know if he was to join the party or not. Dr Smith did not ask him to do so. He was quite unsettled at that time. Witness had no doubt all the story was an imagination; that it was a mere delusion. No Highlanders came that witness is aware of. Dr Smith practised in Montrose about eighteen years; had a good business, and was much esteemed; had also public appointments. I only knew him six years. Witness lived in his house from the time he entered into partnership with him. He frequently spat blood during these eighteen months. This does not go along with the state of the liver, but from the state of the mucous membrane of the throat and stomach. He was restless during these eighteen months, suspicious of the people in his house, and said they were plotting against him; but there was no foundation for that. Latterly he had been jealous of witness. From the time he went to be his partner in July 1853, witness took charge of the business. He (Dr Smith) underwent severe medical treatment; large quantities of calomel, leeching, iodine, outwardly and inwardly, during these eighteen months. Would latterly give no money to clothe his children, though he had it in his pocket. He has five, of whom three lived at home. When the door-bell rang he was very suspicious, and would pull up window and look over. Seemed to have no sensibility of cold or exposure; kept windows and doors open, and went about the house naked. Insensibility to cold is a common symptom of insanity. He got gradually worse; his pulse was generally about 130, twice as quick as it should be; sometimes 140 and 160. Witness does not think he would have acted as he did, in the differences with his wife, but for his state of mind.

Re-examined by the Lord Advocate.—What did Dr Smith say he was accused of by Mr Smith? A. He accused him of improper intimacy with his wife, and witness understood it was this that so much unsettled him. Witness first thought Dr Smith insane at the Haughs of Kinnaird, in December 1853—not before. He had disease of liver before that. This disease does not necessarily bring on insanity; not of itself. Witness first observed congestion of the brain during those eighteen months. This would not of itself imply insanity; but when under it, it implies that the patient loses his reason during the attack, and does not know what he is about. This has been the case with him from time to time during these eighteen months. Witness entered into partnership with him in July 1853. He was not aware of these attacks at that time; Dr Smith has been insane, he thinks, at intervals, during the whole of that period, from congestion of brain, aggravated by the state of the liver. There was something in these attacks of excitement which could not have been produced by drink. Q. What? A. Determination of blood to the head. Q. Why could these not be so produced? A. Because they were too lasting. They lasted a few days at a time. Effects of drink may last for mouths. I say the attacks too long to result from drink. Because I considered them so. It was duration of attack and other symptoms, which showed they were not from drink. His want of power in one limb and not in the other, which indicated partial paralysis. If a man with these symptoms drinks immoderately, that would aggravate symptoms very much. Partial paralysis does not infer insanity. In December 1853, witness had proposed to Drs Steel and Booth that Dr Smith should go to Dumfries Asylum; never after till the Sunday in question. Between September 1853 and January 1854, Dr Smith had seen one or two patients occasionally. Witness had occasionally consulted him during that time on matters of practice, about his (witness') cases. Mr and Mrs Smith slept up stairs usually, but witness does not know where they slept that night. Dr Smith did not say he had taken a pistol with him, but spoke of having pistols both previously and that night. Witness was aware of his application for an appointment from the Colonial Land and Emigration Board, and that he had got it. Never said to him, nor to any body else, that he was not fit to hold it. Witness put his testimonials and his diploma into the bag at his request. He has prevented Dr Smith's seeing patients; not always. He was not immoderately given to drink during the period that witness lived in his house, to his knowledge. He was not a drunkard. Witness has seen him take large quantities of wine at a time. Has seen him come home three or four times what he would call tipsy.

Cross-examined by the Dean.—He was a good deal from home between January and September 1854. Left in January, and only at home a day or two at a time after that. Witness' opportunities of seeing him during that time were small. He was in London one week, and also at Liverpool. Witness put down these absences to his unsettled state of mind. He stated that he went to Edinburgh for health, and to consult medical men there. Considered himself better without him for practice, and did not call upon him to account for absence when he came home on these occasions. Witness told him who were the patients he was attending. This is all that he meant by consulting him.

By the Lord Justice-Clerk.—Witness never prescribed wine for him to the extent of a bottle and a half per day. He prescribed a glass of wine two or three times a day, and some beer, he thinks. He was very weak. He (Dr Smith) also had brandy under other medical advice. It was given with witness' knowledge. Cannot say to what extent; not above two glasses a day by prescription. Witness was at Haughs of Kinnaird almost daily during his illness there. He was sometimes high and sometimes low. Mrs Clark was employed as a nurse; she came to his house July 1853, soon after witness joined him. She was recommended by Mr Smith. Mrs Clark came first as a servant. On his visit to Noranside in the end of August or beginning of September 1853, he remained five or six weeks there as an invalid. Mrs Clark was sent for, and went to attend him as a nurse; she was sent for by Miss Carnegie. Miss C. is no relation, but an old acquaintance. Witness never saw pistols in his possession; he believes he had none. He heard of no pistols in passing through Brechin. On a previous occasion he spoke of pistols with reference to the Smiths, and said he had pistols with him at the Haughs of Kinnaird; this during last harvest, and before Mrs Smith returned. This was at one of the times he returned accidentally to Montrose. Dr Smith did not say what he was to do with the pistols. Said he had been out there, and had them with him. Witness did not know whether to believe it or not. Cannot say when Dr Smith first mentioned that he had been accused of improper intimacy with Smith's wife, but his (witness') impression is that it was first in harvest, during one of his visits to his house. He mentioned this with much anger, and said he should like to see the man who had dared to cast such an imputation on him. Witness had heard that Smith had made this accusation, and did not consider it a delusion that that accusation was made. Witness sent him no accounts of practice during the summer after July. He made no remarks. He sometimes drew money from me, but very little—a pound or two now and then. I know he got money from friends, such as Miss Carnegie. Witness expended the money on his family; he lived in the house, and kept his household. It was Dr Morrison who first proposed the partnership to witness: he had known Dr Smith for about five years before, but had not been much in his company. Witness began to see he had been brought into the business to keep it together. He had no doubt that when Dr Smith said he had been at the Haughs of Kinnaird he meant on the night of the fire, and inferred from what he said he must have had something to do with it. He said they can do nothing with me. At another time he said, What can they do with me? He did not say why they could do nothing with him. Mr Smart and witness told him he would be apprehended on suspicion. He said, they can do nothing with me; they have nothing to do with me. Witness thinks he meant they are not entitled to take it up against him. Witness' reason for thinking so was what the hostler at the inn had told him (witness) of his coming there at a certain hour on Saturday night, and leaving at ten on Sunday morning, from which he concluded he was innocent, and that it was a false charge. Did not think it worth while to reason with him (Dr Smith) in the state he was in. Mr Smith said Dr Smith had been allowed too much drink in his house, but did not say who had allowed it. Witness heard nothing of his being in the dining-room in his shirt. Mrs Clark kept the key of the press when he lived in his own house. Witness is not aware that she gave him too much drink with a view to pander to his habit. Miss Carnegie objected to his being sent to asylum in December 1853, as afraid that the confinement, and that the idea of it, would kill him. Witness thought it would have done him great service, but he had no right to insist on it.

By the Dean.—Examined chest by stethoscope from time to time, and formed an opinion partly in that way of the state of the circulation.

Charles Somerville, merchant, Montrose.—Witness' house is at Little Mill, about a mile from turn in Brechin road. Has known Dr Smith since 1838, and intimately for the last ten years. He was generally considered sober, but latterly he could indulge freely in company; this was during the most part of the last ten years. When sober he was sad. He used to get very much excited. One night witness thought he was not sane. This was on occasion (November 1853) of a party given to Dr Morrison on leaving the office of surgeon to the asylum at Montrose. Morrison then said that Dr Smith had taken one of his bad turns in consequence of a letter about his wife and family. At times witness thought he was right enough in his mind; at other times the reverse. On Thursday, 21st September, Mrs Smith of Haughs returned from Edinburgh. Witness saw Dr Smith the following Sunday. He complained of Mrs Smith going back without his knowledge, contrary to her promise. He said he had been at Haughs of Kinnaird, reconnoitering, on his way to Little Mill. On the Saturday following, the 30th, witness saw Dr Smith in his house; he was much excited. Witness asked Mr Finlay, who was there about some of Dr Smith's matters, to leave the house, as Dr Smith was angry with him. Witness left between ten and eleven that morning, and went to Driver's with Dr Smith. Returned to Dr Smith's about five o'clock P.M., and stayed till about six. Then went with him up streets to witness' house at Little Mill, which we reached about seven, and he remained till the ten o'clock bell had rung. He got half a glass of brandy when we went in, one tumbler of toddy before supper, and one after supper, which he did not finish. He said nothing that day of Mr Smith of Haughs and his wife. He had a sword and a small dirk with him. (Identifies the dirk). He usually carried it, also a sword for some months. He asked for my horse and gig to go to Brechin and stay all night, after which he said he was going to Miss Carnegie, from whom he always got good counsel. He was dressed somewhat like a sailor, in a sort of flannel shirt, lightish brown trowsers; but when he left me he had on a good greatcoat. He left me a few minutes past ten o'clock. He said that day he had a sore upon his ankle. Dr Smith sent out for turpentine, and got it in a soda-water bottle. He took the sword with him in the gig.

Cross-examined by Mr Muir.—Dr Smith had the best practice in Montrose, and the neighbourhood, when witness first knew him, and was much esteemed as a medical man, which he continued to be till within the last two or three years, when his practice fell off, as he would not attend his patients. Witness observed considerable change in his character during that time. He was not like the man he used to be. On occasion of the party to Dr Morrison, we waited for half an hour in his house for him, and when he came he said he had been out on a drive to Haughs, sat down, and in a jeering, rough way, asked the clergyman to say grace. Took dinner and wine during dinner, and then went to his bed-room, where Dr Officer saw him. After which he came in with nothing but his shirt on, and stood before the fire, and then went back to his room. He returned a second time, with a foolish-like dress; went out and came in at the main door, wearing women's clothes, a cap, cloak, &c., and was very much excited. Witness was much disgusted with his conduct and left, saying he would not go back. Witness, in his house, had heard Dr Smith complain much of his head. Remembers one occasion when Dr Smith should have remained in witness's house attending his wife, that he went out, and was found at the side of the fence by Mrs Burnet, who called two policemen. He got no drink in witness' house that day, to cause anything of that kind,—some porter only. He could not get any drink elsewhere from the time witness left him to go to bed till he was found, as there was no public-house near. This was about two years ago. Never saw him have pistols; but on one occasion, in witness' house, his wife said she saw them. He said to witness, last time he spoke of pistols, he had had, or was to have a duel with a Dr Alexander in Edinburgh, and had sent for Mr Inglis at Lochlie, to bring a brace of pistols with him. This was just after his last return from Edinburgh. He often spoke of shooting or stabbing people. Spoke of having a list of victims. He mentioned Mr George Smart, a much respected man, as a victim; Boyd the banker, and Dr Smith of the Royal Navy, &c. He often, as witness thought, took up groundless prejudices. After his return from Edinburgh, he showed witness large marks on his arm, which he got in Edinburgh, fighting with Dr Alexander. Showed witness blue marks on his arm, which he said he got from Dr Alexander. He was then in an excited state. On the Saturday, when he got the gig, he was in an excited state, very much so. He had had drink that day. He was excited in the morning before witness knew of his getting anything. He had a little brandy that morning before leaving Driver's, and about a quarter glass of whisky. At dinner, he took about a glass of brandy, and when he went to witness' house, at Little Mill, he had what was already mentioned. He was not tipsy when he left witness that night; he could drive quite well. Witness did not see him use the turpentine which was brought.

Cross-examined by Mr Patton.—Witness saw the prisoner Campbell that night when he was walking up streets from Little Mill. Campbell appeared tipsy. Witness thought the meeting accidental. Dr Smith spoke to him. Witness had seen Campbell twice at Dr Smith's before. He knew very little about Campbell, but thought him a quiet, inoffensive man, as far as he knew. The Doctor took Campbell into a close, and witness did not like to stand there, so went and waited twenty minutes or half an hour in a shop. This was about six o'clock. When on the way to Little Mill, witness went, after waiting for him at the inn, and found Campbell on a sofa in Dr Smith's study. Witness then went with Dr Smith to his house at Little Mill. The sword was displayed both before and after supper. This was in consequence of witness saying when he spoke of fire-arms, &c., that it was all nonsense, and he had none; he immediately took out the sword, which came right down on the table between witness and his wife. He had previously carried the sword inside of his greatcoat. He said if he met George Smart, he would run him through with it. He was a man of strong likings and dislikings. Did not use it in a threatening way towards witness' wife.

Re-examined by the Lord Advocate.—Remembers that steak was ordered for Campbell in the inn, and one shilling asked as price for it, when the Doctor said, not to give him strong drink, but tea or beer, and accordingly he paid 2s., and we left Campbell there.

William Gray, apprentice to Alexander Mill, Perfumer, High Street, Montrose.—Witness sold to the prisoner Campbell, the stick now shown to him, on the 30th September last, in the evening. It was then the usual length of a walking stick. He asked for a stick—the price was 6d.

John Henderson, inn-keeper, White Horse Inn, Montrose.—Dr Smith came to witness' house, 30th September last, a little past six in the evening, with the prisoner Campbell. Dr Smith asked witness to get a steak ready for Campbell, as soon as possible, but to give him no drink. Witness suggested to give him tea with it. After giving the order Dr Smith left; Campbell left in about half an hour after having got tea and a steak. Nothing in Dr Smith's appearance struck witness that night in the smallest degree,—he was very quiet. Dr Smith and Campbell were in a room together for a few minutes. Mr Somerville went in after this, and then went away with Dr Smith, leaving Campbell.

James Anderson lived at Leuchland Toll-bar in September and October last. The bar is about a mile from Brechin. Mr Somerville's house at Little Mill is nearer Montrose. Witness recollects Dr Smith passing through the bar on the night of the 30th September last, in a gig, belonging to Mr Somerville, Little Mill, about 11 P.M. Campbell was in the gig with him. Witness saw fire the same night, or rather early next morning in the direction of Haughs of Kinnaird. It was a quarter past one when witness saw it. The Haughs is from two to three miles from the toll-bar, but is not on the public road. From the toll-bar to Brechin is one mile.

James Wilson, hostler, at the Commercial Inn, Brechin, last September. Witness recollects Dr Smith coming there on 30th September last, about five or ten minutes before eleven o'clock, P.M. He was driving a gig. Nobody was with him. Witness put up the horse and gig for the night, by his desire. Witness did not see him again till next morning at a quarter before ten. He left at that time. The night before, he said he was going to Noranside, and in the morning he went in that direction. It is four miles from Brechin to Haughs of Kinnaird. Witness saw nothing particular in his behaviour that night. He was quite cool as far as witness saw.

By the Lord Justice-Clerk.—Nothing passed but what has been mentioned.

Ann Steel, servant in the Swan Inn, Brechin, in September last. Dr Smith came there a little past eleven on the night of 30th September with Campbell. Campbell had in his hand the stick, of which a part is now shown. Dr Smith asked for a bed. He did not ask for spirits from witness, but she heard Dr Smith ask for spirits. He could not get it, as it was past the hour. It was before this he asked for a bed. After this they both went away.

Cross-examined by Mr Patton.—Campbell stood in the doorway where there is a lamp.

James Fleming, boots in the Commercial inn.—Dr Smith came at a quarter before three in the morning of 1st October last, to the Commercial Inn, Brechin. Witness let him in; he saw nothing particular about him. There was sweat running down his face—his boots were very dirty. Witness cleaned them next morning; they were dry by that time but very earthy, and seemed to have been wet. His trowsers appeared to have been wet too, and dirty with earth. Witness saw his stockings next morning about half-past nine; they appeared very dirty and rather wet. When he came in at three o'clock that morning, he went into the pantry and had something to drink. He spoke in the morning of having fainted and sat down on the street, and got some money taken off him, about a pound or 30s. He said he had been visiting acquaintances or friends; gave no names. His dress indicated that he had been going through ploughed fields, *i.e.*, earth adhering to boots and trowsers as if they had been wet, and earth had adhered Witness saw him go away at a quarter before ten next morning. Before he went away, he showed witness a dirk, now identified, and said that, for a person visiting about at night, it was very handy, and said he sometimes carried a pistol.

Miss Helen Hay Carnegie.—Has known Dr Smith many years, about seventeen years. He was her medical attendant as long as she was at Noranside. She is aware he was acquainted with Mr and Mrs Smith, and that they had some differences, in consequence of which Mrs Smith went to Edinburgh. Dr Smith complained to witness that she came back without his advice, and thereby he was insulted. He told me Mrs Smith had applied to him about a certificate, and had gone back without his advice. He spoke of a letter he had sent; this witness thinks was on 27th of September, being Wednesday before the fire. Witness told him she thought he had done wrong, although he had not told her what was in it, but she understood it was a reproachful letter, and pressed him to write an apology. He said he would, and sat down to write it, and then threw away the pen, saying he expected to find an answer from her when he went home. He left witness' house next day, and she saw him again on Sunday the 1st October, about ten o'clock A.M. Noranside is about eight miles from Brechin. He told witness he had burned the stack-yard of Smith at Haughs of Kinnaird. In the course of the day, he told witness he had done it by means of a lucifer match. He said he had a pair of pistols hid at Haughs of Kinnaird. Sometimes understood him to say something about having three pairs of pistols. Dr Steel, Dr Officer, and Mr Smart came that day. Dr Steel was in attendance on her brother, and came on that account about mid-day—Dr Officer and Mr Smart after that—she thinks not very long after. She had some consultation with Dr Steel and Dr Officer, as to what Dr Smith should do, or rather as to what should be done with Dr Smith, who said he was to return to Haughs for his pistols, and to take back the carriage he had borrowed of Mr Somerville. Dr Smith was not present at those consultations. Dr Smith proposed to go to Dumfries as nobody would think of finding him there. He had been unwilling to consent to go anywhere except back to Montrose. Witness urged him not to go back to Montrose. He said if he did not go it would look like guilt. Ultimately he went away with the view of going to Dr Brown at Dumfries. He said he expected to have burned the house at Haughs of Kinnaird, but the wind had changed and carried it off the house. Shown the letter; witness thinks she got it on the Tuesday after the fire. The post-mark was Coupar-Angus, October 2d, addressed to Miss Carnegie, and signed by Dr Smith.

Coupar Angus, Sunday Night.

MY DEAR MISS CARNEGIE,—Here I am so far all right after a cold ride. I shall be on the out-look by the morning. I had much difficulty in going west instead of east. There I feel I must be yet.

I don't understand Dr Steel. He perhaps understands me; but he is mistaken.

I shall proceed direct to Dr Brown's in the morning, whatever comes in my way. Send Mrs Smith there, it shall be my rendezvous for the present, but I have no intention of *remaining there if you please.*

Should you have occasion to write Officer, will you kindly tell him to send my or rather your plaid with other things to Dumfries. I did not mention it in my pencil note to him.—Ever yours, with my sincerest blessing.

(Signed) GEO. L. SMITH.

Addressed Miss Carnegie.

Witness did not see him again. Dr Smith told witness that he had a friend with him at Haughs of Kinnaird who had deserted him, and that this was Campbell.

This answer taken at Mr Patton's request.—He, Dr Smith, said no person saw him do it.

Cross-examined by the Dean.—The consultation was short, because of Dr Smith's state of mind, which was excited and jealous. Witness was afraid of giving him offence. He said he had loaded pistols—one brace decidedly he said he had; but at one time he mentioned three brace of pistols, and that they would fire twelve shots. He did not say how he was going to use them. He was staying with witness in September 1853, when Dr Christison saw him. After that she saw him at the Haughs in December, when he was very ill. She first observed this illness in January 1853. She had known him seventeen years intimately. She had a friendship for him. When she lived in Montrose he was a young man and had not so much practice. After she removed to Noranside she did not know so much about his practice. He was greatly esteemed for his moral qualities and his benevolence. A change very marked and sad appeared in him in January 1853, and it got gradually worse. He was ill apparently in bodily health in January 1853. The moment food was brought into the room he became sick at the sight or smell of it, and had to leave the room and vomit. Took most violent shivering fits, such as witness never saw before, and mostly kept his bed. Troubled also about his domestic affairs. In July following, *i. e.*, 1853, witness saw Dr Smith in Edinburgh. His mind was much excited in consequence of Mr George Smart's saying to the Episcopal clergyman of Montrose that he (Dr Smith) was mad. He lost composure in a way witness never saw before, and spoke of forcing apology from Mr Smart. He had previously attended Mr George Smart's family. Witness then first became impressed with the conviction that the person on whom she was accustomed to lean must, for the future depend on her for subsistence. In September, when the doctors visited him, he was much worse, and had a fresh attack in December. His mind much worse in September than in July. He showed a change, a want of tact—would repeat to servants what I had mentioned to him as to domestic affairs. Formerly he was a person of discretion, but by this time this was a good deal gone. When witness saw him in December at Haughs, he was not fit to converse. She sat up with him one entire night. He was not in delirium. She saw him a good deal in the course of the following year, when he lived a good deal with her at Laverock House, near Edinburgh. Showed then fierce resentments from causes altogether inadequate. Often dreaded, when he left her in the morning that the first thing they would hear would be that he was arrested for some act of violence. He seemed to have lost all control over these passionate feelings. Witness must have seen him in August before she went to Noranside; only saw him on the two occasions mentioned, after that.

Re-examined by the Lord Advocate.—He left witness generally in the morning at Laverock Bank, and usually returned to dinner about half-past four. He had no employment at that time. His conversation was rational on other subjects than these inadequate causes of offence, and then conversed quite calmly. He had no delusion that she is aware of; did not wander in his speech. He was an intelligent and well-educated man.

Question by the Dean.—Was he given to intemperance? A. Witness never saw him what one could call tipsy. He was never disagreeably affected by liquor. In September 1853 he heard of a medical man applying through Boyd the banker for situation in Montrose, in the idea he was dying, after which almost his sole aim and object in desiring to recover was to challenge this gentleman, *i. e.* Boyd. Mr Goalen at Laverock Bank had a dog which howled, of which witness complained, on which Dr Smith was for going to Mr Goalen in a violent manner, which made her afraid of complaining again.

By the Lord Justice-Clerk.—When he told witness about firing the stackyard, she met him on her way between the house and the offices before he came in, and he immediately told her of having fired the stackyard, looking wild, but not much wilder than on the previous Wednesday. His words were I have set fire to the stackyard at Haughs, but she cannot be sure of the words. She said, "Oh, Dr Smith, Dr Smith." She believed it from the state she saw him in. He said in reply immediately, What do you think they'll do to me? Do you think they'll banish me? Afterwards he talked during the day of going to Australia, as if this was nothing to come in his way. In a short time Dr Steel came in to see witness's brother, but not into the room where Dr Smith was. She told Dr Steel she thought she would be the first person Dr Smith would confess a crime to, because he leant on her. He had no conception it was a crime. He asked witness whether he should confess it, implying that he did not like to tell an untruth. Her answer was, "I think you had better not confess, because that cannot be recalled." He seemed, in fact, to think very little about it. Witness thinks he was more agitated than usual at this time. He had luncheon in her house, then came to dinner. He seemed quite collected and composed at dinner. The brother of witness, who was an invalid, was at dinner, and she did not wish to agitate him; he previously was perfectly unconscious that any thing had happened. She was not present when Dr Smith spoke of it to Dr Officer, but Dr Smith said to me in the evening he thought he would tell Officer, but he could not tell Mr Smart, as he was a Justice of the Peace, and would be bound to apprehend him. Witness discovered nothing which brought him to her house that day, except to tell her this occurrence. She was so shocked and distressed that she asked no questions. He told her he had spent the night in Mr Bean's Inn, the Commercial Inn in Brechin, and that he had slept soundly; indeed, had overslept himself, as he had intended to be with her earlier. Witness had been all along cognizant of his application for a ship, and on the Sunday he showed her the letter conferring the appointment. He had not got it on the Wednesday. Witness had offered to assist him with his outfit before this. This subject was not mentioned on the Sunday morning. Q. Are you quite satisfied that that Wednesday, when he came to you, he was entirely deranged. A. I am quite satisfied he was an irresponsible agent.

William Milne was waiter at the County Hotel, Forfar, in September and October last. Dr Smith came there in a gig with Dr Steel on Sunday night, October 1, about 11 o'clock. Dr Smith ordered a carriage to go to Coupar-Angus to see a friend. Witness said that there had been a great fire at Haughs of Kinnaird. He said he had heard something of it. There was nothing peculiar in his manner at that time. He left at a quarter or twenty minutes before one o'clock in the night, *i. e.*, he came about eleven at night of the Sunday, and left less than two hours thereafter. Miss Carnegie's house was about nine miles off.

John Beattie drove Dr Smith from Forfar on Sunday night or Monday morning, the 1st and 2d October; left him at the Strathmore Arms, Coupar-Angus, about 3 A.M. of 2d October.

James Fleming, innkeeper, Strathmore Arms Inn, Coupar-Angus.—Dr Smith arrived here about 3 o'clock, A.M.. of 2d October. He got a glass of whisky as he said he was cold. Then he had supper and toddy; he asked witness to join him; he did, and sat with him about three-quarters of an hour—talked of shooting, fishing, and sporting very pleasantly and quite coolly and collectedly. He got Railway Time Table, and noted down hours of starting of different trains; said he was going to England—he thinks to Liverpool. Dr Smith went to bed, having told the chambermaid to wake him. He told witness he wanted to go away by the first train. The Procurator-Fiscal and two policemen arrived before he was up, and took him into custody. Witness' house is in Perthshire. The officers were Perthshire officers.

Jean Duncan, chambermaid of the Strathmore Arms, Coupar-Angus.—Remembers Dr Smith coming very early on the morning of 2d October. He told witness to call him for the first train. He did not get up, and officers came and apprehended him.

Isabella Duncan, lived with Robert Gordon, blacksmith at Pow-mouth last October. The Pow runs into the south Esk, passing Haughs of Kinnaird. Witness found the stick, now shewn her, below Pow-mouth a quarter of a mile or so; she gave it to her master.

Isabella Baird, in October last, was servant to John Henderson, White Horse Inn, Montrose; saw Campbell on the night of Saturday 30th September, having beefsteaks and tea in the inn. Witness saw him again on the 10th October. He told her, if she had given him as much whisky as she had given him tea, he would not have been able to go on such a case; he said he went to Brechin to fetch back Mr Somerville's pony; said nothing about Haughs of Kinnaird, nor the fire. He said Dr Smith invited him to take a walk till the pony fed, and they went to the Bridge of Brechin, and the Doctor took a naked sword in one hand, and him by the other, and told him to go on—said nothing more.

Robert Scott, farmer at Balwyllo.—Went for fire-engines on the night of the fire, 6 or 7 miles from Brechin on the road, he met a man who asked if he was going for the Montrose engine. Witness said yes. He said "you need not go, for they have got word already" this was nearly at 4 o'clock A.M. on Sunday 1st October, and about three miles from Haughs of Kinnaird. Witness turned back in consequence, and the Montrose engines never came—cannot say that Campbell was the man; would not swear to him.

Alexander Herd, officer of Montrose police.—Had charge of Campbell on 26th October, after he was apprehended. Q.—Did he tell you anything?

Mr Patton objecting to the question, and the witness being withdrawn, said that Campbell was not apprehended till the 26th October, when he was going to emit a declaration, which was dated 27th October, that it is not the province of police officers to examine: that Campbell was previously precognosced as a witness, and never was apprehended as a party till on his way to Forfar to emit a declaration.

A. Herd's examination resumed—He did not know Campbell was examined as a witness before this—did not know he was on his way to Forfar to emit a declaration. Witness did not take him till 26th October. It was in the police office he had the conversation with Campbell, in Mr Brownlee's room about 4 o'clock P.M. Mr Brownlee came in, and put questions to him about 11 at night.

By the Lord Advocate.—Witness thinks conversation began by Brownlees' putting questions.

The Lord Advocate withdrew the witness.

James Brownlee.—Has the charge of the fire-engines in Montrose. He is superintendent of police; no message was sent there for the Montrose fire engines on the morning of Sunday 1st October.

What the witness has to say, is what he heard in answer to questions put by Mr Brownlee.

Michael Hinch.—Is superintendent of police of Forfar. He examined the person of Dr Smith, after he was apprehended. Found the dirk and sheath now shewn, on him. Also piece of the match-box shewn, and a letter from the colonial land and emigration office, now shewn.

John Morison Milne—Holds commission as extra procurator-fiscal of Forfar. In consequence of information went to the hotel in Coupar in Angus, and got Dr Smith apprehended about half-past eight in the morning. Witness had an officer and two assistants. He told Dr Smith the charge against him, and that these persons were officers of law, that any statement he might make to them might be used against him, and not in his favour, and that it was for him to consider whether it was prudent to say anything or nothing. He said he knew nothing of the charge, and thanked him for his warning. He seemed disappointed at being stopped, and shewed him a letter from the colonial land and emigration office, and said that it was in consequence of it, that he was anxious to proceed; Dr Smith wished bail or deposit to be taken, and to allow him to proceed. Witness said he could do nothing but take him before the Sheriff for examination. He asked if he could see a law agent. Witness said he could not do this then; he behaved in a very quiet, gentlemanlike manner. Witness went to the station, and saw him claim a bag, as his own. It was addressed to Miss Clark; he said—"that is my luggage, and is all right, it is addressed to my housekeeper's daughter." When he was in the hotel, he repeatedly applied to be furnished with whisky through the landlord and the girl in attendance. There was nothing in his manner or conversation to indicate anything unusual about him.

Robert Smart—Went to Miss Carnegie on Sunday 1st October. The charge against Dr Smith was talked of in his presence; he neither admitted nor denied it. He spoke of Mr Smith of Haughs, in a very violent manner. He said "how can they prove it against me?" Witness does not recollect his mentioning the cause of his irritation against Smith. Witness gave him a certificate on the Monday previous to the fire, of his fitness for the office he wished from the colonial land and emigration Board.

By the Dean.—Witness would not have given that certificate after the 1st October. From his general demeanour and violence thought him insane; had never seen him in such a state before. He used very violent language. He saw him in the presence of Miss Carnegie and Dr Officer. He was much more like a maniac than a sane man. He was very wild, and asked if Smith was burned? He said burning was too good a death for Mr Smith. He asked if the houses were not burned, and regretted they were not. He was completely altered. Witness had known him many years, and had not seen anything of this kind before. He saw him the Monday before, when he was quite quiet; had seen him once or twice during the summer. All the time witness saw him at Noranside he had the appearance of being insane. Witness had luncheon there. He had quite that wild appearance at luncheon-time, but was quieter then. He should not say he spoke quite rationally; he began some badinage about witness and a young lady. Q. Was there more than the excitement which would be natural if he had committed such a crime? A. Witness thought if he had committed such a crime his appearance would have been different—that it would have evinced some terror. He did not confess the crime to witness. He has known him many years; he was always rather an excitable man. He had a quick temper, and, when excited, very violent. Not to my knowledge, more given to drinking in the latter months of my acquaintance than before.

By the Lord Justice-Clerk.—When he expressed regret that the houses were not burned, did he seem wild in manner, stamping about the room, raving, and talking very loudly? A. Witness had a doubt as to his committing it, for he thought him incapable of such a thing; had some doubt after all he said about the houses, &c., not being burned. It was his rage at Mr Smith which moved him to excitement. Witness does not know the cause of his resentment. When Dr Officer said he was suspected, he said, how can they prove it? Do not remember that he expressed any indignation at the suspicion. Witness was not taken into consultation as to what was to be done with Dr Smith, but heard occasionally what was going on. Witness does not recollect giving any opinion, but from Dr Smith's conduct he thought he was not fit to be at large. He saw a great change in Dr Smith the last year and half; thinner, and looking ill. Cannot say he saw any other change, but he saw little of him. Mr George Smart is witness' uncle. On the Monday Dr Smith spoke to witness about something which enraged him against witness' uncle; he accused his uncle of having taken liberties with his wife, which witness disbelieved, and was very much surprised at, and said was not possible. He would, he said, write to his wife and get particulars, and show them to witness. Witness took up no distrust of his sanity from this; he thought it just an unjust suspicion, and still thinks so.

John Burness, surgeon in Montrose.—Took some charge at one time of Dr Smith's patients for him; about two and a half years since. He was always exciteable and violent when he was provoked. Never saw any symptoms of insanity about him. Witness has not the least reason to doubt his sanity; his means of knowledge come down to about eighteen months ago. He was very fond of company, and witness dares say he indulged rather freely occasionally; thinks he indulged to an extent to affect his health. He generally indulged in violent threats against persons he thought had injured him. Witness has known him four years, but has not spoken to him the last eighteen months. Cannot say he observed any particular change in him. Did not observe anything very remarkable in him. Never saw anything about him to think he was or might become insane. Witness is not aware that he ever expressed that opinion; he thinks he would have been aware if he had. He lived for some time with Mr Somerville. May have said of Dr Smith, as of any other person, he might become insane; he never expressed any such serious opinion; he is not aware that he ever expressed that opinion to Mr David Scott. He does not think he would have forgotten it if he had. Q. Do you recollect conversation with Mr David Scott in front of the police-office in Montrose? A. I do not recollect any such conversation. I am in a condition to state I did not seriously express an opinion that Dr Smith was insane. After the fire took place witness might have said something of the kind, but not meaning to express any such serious opinion, and does not recollect using any such expression. Witness has seen Dr Smith indulge freely; has seen him the worse of liquor; not perhaps drunk. Cannot say when this was, but it was within a year of the time my intimacy with him ceased. Dr S. was in his own house, and a party present,—witness thinks Mr Somerville, Mr Bain, surveyor of taxes, Mr Driver, and himself; does not recollect of any others. Does not recollect of anything very remarkable happening that night. As far as witness knows he was sober himself; he does not recollect how he got home that night. Does not recollect of Dr Morrison being there. He was so often in Dr Smith's, cannot remember particulars. This is the only occasion witness recollects distinctly of Dr Smith being the worse of drink.

By the Lord Justice-Clerk.—What caused the cessation of acquaintance? He told me he was to send away his wife. We had some words about that. He thought that was it. He knew his alleged reasons, but thought them not true. He resolved, and then they had words about it. After that they passed each other.

Tuesday, 16th January.

Alexander Smith, surgeon to prison of Forfar—Remembers Dr Smith being brought there on the 2d October last. Remained till removed for this trial last week. Had frequent opportunities of observing his state of mind. When first admitted to jail he was labouring under considerable excitement. His stomach was much disordered, which witness ascribed at the time to too free an indulgence in liquor, and partly to the position in which he found himself. Subsequent observation of him led witness to think that conjecture right. The excitement subsided very much the following day—he was then much more composed and quiet. Saw him on an average fully twice a-week all the time he was there, frequently for a couple of hours at a time, he spoke of his previous history, health, and temperament. Witness had known him before very slightly. Everything he said to him in a quiet and rational manner Dr

Smith, stated that about 18 months previously he had fallen into bad health, which he ascribed partly to family vexations that his right leg was partly paralytic, he laboured under partial derangement of liver, said he had naturally a very irritable temper, that he was easily irritated, particularly since his health became bad. In all these conversations, witness never perceived any mental aberration or delusions of any kind; period of witness' observation from 2d October to the beginning of last week. Witness considered him a sane man, has no doubt of it. Q. If for 18 months before the 2d October he had been becoming gradually more and more insane, do you think you could have found the symptoms you speak of? A. I think it exceedingly unlikely. Q. If told during that period he was liable to paroxysms of rage, and you found these had during these four months entirely ceased, would that strengthen the belief that they were caused entirely by drink? A. It would indeed. In that case cessation of paroxysms when drink was not to be had, would be exactly what I would have expected. During the whole period of his confinement witness heard of no such paroxysm of temper. Enlargement and disease of liver do not imply insanity nor mental excitement; nor does congestion of the brain or paralysis of limbs necessarily imply insanity. It is nothing uncommon to all three without any mental derangement. If he had been in a state of insanity, in which he was liable to delusions, does not think it likely these delusions would not manifest themselves in prison, unless they arose from drink.

Cross-examined by the Dean.—His bodily health was in a very unsatisfactory state—have doubts if brain was in healthy state; very difficult to say what diseased action of the brain arises from. Witness begged to decline giving a decided opinion whether the brain was at all affected or not. Dr Smith has enlargement of the liver, which can be felt; it seems to witness a simple case of enlargement of the liver; nothing else. He should reckon it quite curable; he was liable to bleeding of gums, which indicates general weakness of the system, that generally arises from imperfect assimilation of food, inducing weakness of bloodvessels. His digestion was irregular, at times pretty good, at other times very defective. Witness does not consider that he had any disease of the mucous membrane—he once or twice applied the stethoscope to ascertain the state of the circulation. The action of the heart is too frequent, and beat unequally; a strong beat was succeeded by a weaker one. At times he was subject to palpitation, said to be worse during the night. Witness rarely found his pulse under 90, he has found it 110; does not remember ever finding it more; thinks there is functional derangement both of the stomach and heart, and no organic disease, except of the liver, that he could discover; indigestion is a very prolific cause of functional derangement of the heart; he attributes the latter to indigestion. He never could satisfy himself that there was disease of the brain. Witness once bled him since he came to him in October. Indigestion affects the mind very much, it renders a man very irritable, does not think that any of Dr Smith's bodily ailments were calculated to produce insanity. Q. Suppose we were wrong in ascribing his excitement to intoxicating liquors, to what would you ascribe it? A. It might arise from indigestion, acting sympathetically on the brain, not directly. Can find nothing else than what has been mentioned to account for mental excitement in Dr Smith.

Re-examined by Lord Advocate.—State of the brain is often very occult—the disease of the liver would certainly be aggravated by drinking. Witness never saw any excitement in Dr Smith, except the first day.

By the Lord Justice-Clerk.—It was about 3 o'clock P.M. of Monday 2d October, witness first saw Dr Smith. He was not drunk when he was brought to the jail—he had quite the appearance of a man after a severe debauch, such as a day or night, or two days before. Witness had at that time received no information of his having fallen into habits of drinking; he got information to that effect about a month after he was brought to the prison; his opinion was previously formed. Witness first recommended some aperient medicine, which he believes was administered. Next morning when he saw him, *i.e.*, 3d October, he was much more quiet and composed; does not remember whether, on Tuesday, he was asked if Dr Smith was in a fit state to be examined. He was asked that question at some period; he had been once examined before witness saw him on Monday, 2d October, at 3 P.M. Witness thought the appearances very decided, and marked of his having come out of a recent debauch. He did not speak incoherently, but there was a restlessness about him, and he was talking loud; he said something as if the officers had treated him with indignity. The white of his eye was yellowish, and the bloodvessels a little turgid. It did not strike witness there was any wildness or excitement in the eye; does not recollect anything else than what he has mentioned, as particularly indicating excitement. Witness rather avoided speaking of the fire-raising—nothing passed indicating how he viewed it, that witness remembers. Once he alluded slightly to it—he asked witness "What will they do with me?" Witness saw no appearance, while in jail, of Dr Smith's wishing to feign derangement.

By the Court, on suggestion of Lord Advocate.—Was excitement, when you first saw him, such as would make him not know that day what he was doing. A. No; I should say he was in a fit state to undergo a judicial examination when I first saw him. Q. Suppose he had passed two sleepless nights, and swallowed a considerable quantity of spirits, would that have produced the state in which you found him first? A. Certainly it would.

By the Court, on suggestion of the Dean of Faculty.—Witness gave authority to the keeper to give him stimulants, if necessary. Witness was somewhat afraid of delirium tremens. It did not ensue. He believes Dr Smith only once or twice got any stimulants—he is aware that he declined these afterwards.

By the Justice Clerk.—Witness is not aware that disease of liver gives thirst for ardent spirits.

Dr William Malcolm, Perth.—Is physician to the Perth Asylum, and experienced in the treatment of lunatics. He visited Dr Smith in Forfar prison four times, 22d November, 27th November, 8th and 19th December. During these visits, first time he remained half an hour, second and third times upwards of an hour, and last time above two hours. Took notes of what passed immediately on return home, which he has here. Here Dr Malcolm read his notes, as follow:—

November 22, 1854.—By desire of Crown-Counsel I this day went to Forfar and visited Dr George L. Smith in the prison there. I met Mr Forsyth on my arrival, and shortly afterwards Dr Smith of Forfar, and from them learned that Dr G. Smith had the character of being an eccentric and at times a violent-tempered man. I accompanied Dr Smith of Forfar to the prison, and was introduced to Dr G. L. Smith, and had conversation with him for about an hour. He mentioned that he had been acquainted with my son and had gone over the Perth Infirmary with him some years ago. That for sometime previously he had suffered much mental anxiety, which had deprived him of sleep—that he had taken much exercise, such as fishing, &c., in order by enduring fatigue that he might sleep more—that he would have slept on the ground or any where when sleep came upon him—he has suffered from enlarged liver, and some months since he had excruciating headaches, for which he applied leeches to his head with relief—that some months since he had an attack of headache, during two days of which he had no recollection of anything which occurred to him, although he has been told that he had spoken sensibly during the whole time—that at this time he did not care what became of him, and upon my asking him if he had ever entertained any thoughts of suicide, he said, "certainly not, far from it"—that he is now in tolerable health, excepting that confinement enervates him—he says to me expressly, that he did not take drink to excess, and never had done so. He spoke of a Miss Carnegie with whom he is intimate, and recapitulated all the conversations he had had with her, and what she had kindly done for him. His memory appears to be good—he is consistent in all his conversations—he appears deep and depressed, but there is no appearance or sign of aberration of intellect; and I would say that Smith, so far from being insane, is an acute, clear-headed man.

November 27, 1854.—I went to Forfar this day to visit Dr George Lillie Smith in the prison there, and on my arrival I met Dr Smith of Forfar, by appointment. Dr Lillie Smith received me most politely; and after some conversation, he said, "I believe you visit me by desire of the authorities." I answered in the affirmative, and no farther allusion was made on the subject. In the course of conversation, I said to him that I found he was intimate with Dr Browne of Dumfries; he said, "Yes," and that the doctor was to visit him some day soon. I said I would be happy to meet Dr Browne, and wished to meet him on Dr Lillie Smith's case. He said he would write to Dr Browne in order that we might arrange to go to Forfar together. Dr L. Smith told me he had been a candidate for the Surgeoncy of the General Prison, and would have succeeded had the first arrangement for medical attendance been carried out; but this being changed he had not got it. I mentioned to him that his face was familiar to me; but I did not recollect where I had met him. He said he had been in my house, along with my son. He seems to be of a strong hemorrhagic diathesis, as his lips are discoloured with blood from his gums; and whilst speaking to me his gums bled a good deal; for this he is taking Tt. Mur. Ferri—he told me that Mr Mein of Edinburgh had pulled a tooth for him, and that the bleeding from the gum had lasted several days, so as to enfeeble him greatly, and that he had derived much benefit from the nitrate of iron prescribed by Dr Begbie of Edinburgh—that on the occasion of the bleeding from the gums, Dr Begbie had been so much alarmed with it as to propose to send for Professor Syme, but this he (Dr Lillie Smith) had declined. He again recurred to his violent headaches, which, during the time they lasted, deprived him of control over himself. His conduct and conversation during this interview, impressed me more and more that he is not an insane man—he is well informed on all subjects we conversed upon—consistent and rational in all matters; and during the time I staid with him he gave me a copy of his certificate when he was a candidate for the General Prison, and also when he was candidate for the situation of President Medical Officer of the Montrose Asylum. These certificates bear date 1848 and 1838 respectively, and all are written by first-class medical men, and all speak of Smith in the highest terms as an accomplished and able medical man.

December 8, 1854.—Went to Forfar and visited Dr Lillie Smith. I found him lying on a sofa and not looking well—he told me he had headache with general derangement of the system, for which Dr Smith of Forfar had ordered leeches to the nape of the neck, and blue pills—that he slept badly, felt cold, urine high-coloured, scanty, and depositing a copious sediment; appetite bad, P. 104, tongue foul. He began the conversation by saying that he would give me every information I wanted about himself and his previous history, and desired me to ask any question I wished. I told him I had abstained from asking questions about his crime in respect for his feelings. He gave me a long history of his ailments for some years—told me had gone to London for the purpose, and with the intention, of going out to Australia in an emigrant ship—that he arrived on Saturday in Leith, and on landing, heard of some very disagreeable circumstances which made him feel sick and irritable—that he went to Montrose that night, and went about his business as usual—that he did not drink, and never used drink but as a gentleman, and became angry and excited at the notion of the public having laid such an imputation upon him. I staid an hour with him, and during the whole period I found his conversation and actions those of a sane man of mental accomplishments, and an acute clear mind. When I spoke of Dr Browne, he said he had had a letter from Miss Carnegie which he read to me, telling him that Dr Browne's visit had been put off till such time as matters should be arranged for the proper time of the doctor's visit.

December 19.—Went to Forfar in consequence of a question sent for me by Crown-Counsel, wishing me to certify of the state of Dr Lillie Smith's mind at the moment he set fire to the stacks. As the putting of such question would involve a confession of guilt, I declined to put it being aware that the unfortunate man would answer me explicitly, and I did not choose to take advantage of his reliance upon me as a gentleman. I found his health deteriorating, he looks paler and more anxious than he did, says he sleeps ill, appetite bad, liver increasing in size—he told me Dr Browne from Dumfries had been to visit him, and had told him that a plea of insanity was to be made for him, and recommended that he (Dr Smith) should place himself in the hands of his lawyers. He was very indignant at Browne for his suggestions, and said he would not submit to be called insane, when he had no insanity about him. He asked if I had reported on his case, and upon my answering in the affirmative, he expressed a wish to know what that was. I told him, and he was much pleased with my opinion. He asked me what the consequence would be of his being found insane, and expressed the greatest abhorrence of the idea of being shut up for life with madmen, and said he would prefer being shot. He asked what I supposed the sentence would be if he pleaded guilty, or was found guilty. I said it might be a certain number of years' "penal servitude:" that with such good friends as he had he would be confined a year or so, and that upon petition he would be liberated and allowed to expatriate himself to the colonies. He said he would willingly accept of this. I at the same time told him that he should take the opinion of his lawyers and not mine, as they knew better than I could what he ought to do. He said with certain limitations he would do so, but that he could not bear the idea of sitting to hear evidence led of his insanity, being conscious all the time he was and had all his life been sane. He said he may have been delirious at times from distress of mind, but certainly never insane. I staid with him upwards of an hour.

Q. Have you any hesitation as to your opinion of his mind? A. No. Saw no symptom of insanity about him of any kind. He knew he was in prison for crime, and probable punishment, and the probable result of plea of insanity. A man could not have spoken more naturally than he did on the subject. He knew right from wrong perfectly well, and was quite aware of his moral responsibility. Witness understood he had gone to London to get the situation of surgeon in an emigrant ship, and that he had got it. Congestion of brain is a disease of which the traces are not to be observed with certainty during life. Where it exists, it does not necessarily produce insanity. Witness found no symptoms of such congestion when he saw him. Nothing in the account he gave him would lead to the conclusion he had congestion of the brain. The headaches might have been nervous or not. Drinking and mental distress, and want of sleep, would aggravate disease of the liver. Witness has known many persons subject to paroxysms of temper and unjust prejudices against persons on false or trivial grounds, though quite sane. Quite common in real life. If he had been growing gradually more insane for eighteen months, he could not have been in the state witness saw him in. Nor could he have been in that state if he had been recently insane. If it commenced in July 1853, and came to a height in September 1854, it is quite impossible he could have been in the state in which he saw him. Mental excitement, want of sleep, and drink, acting on a temper naturally excitable, would produce paroxysms of rage. Witness had found that he had been liable to such paroxysms for eighteen months, and now not being subject to them, witness would at once conclude they had been caused by drink. It frequently happens that persons are in the habit of drinking, but are not much known to drink; that is a matter of observation. Frequently symptoms of excitement are visible in such persons who drink, although not observed to do so. Witness has no great belief in monomania. He thinks if a man is insane on one point, he is insane generally.

Cross-examined by the Dean.—Did not get much information of his bodily and mental health. Judged chiefly from what he told him, and his own observation. Witness did not observe symptoms of drinking, but would not expect that, from the time he had been in prison before he saw him. Dr Smith told him of the attack of 1853, which resulted in paralysis, after the attack of two days headache, when he was insensible. Witness had no meeting with Dr Browne about this case. If there be insanity, he thinks it will appear in so many ways that it will be plain it is there. A man may be more insane on one subject than another.

Re-examined by the Lord Advocate.—Insanity may exhibit itself more on one topic than another.

By the Lord Justice-Clerk.—Witness has known cases of persons who were insane, and wished to be thought sane? Not of persons feigning to be insane, who were angry at being thought insane.

Charles Dickson, Esq., Sheriff-substitute of Forfar.—Dr Smith was brought before him for examination on 2d October last in the afternoon—thinks it was between three and four o'clock, is not exactly sure. Examined into his state before putting him under examination, and formed the opinion that he was in sound and sober senses. Explained the charge to him, and took judicial declaration of 2d October 1854, now shown, after the usual caution. It was emitted freely and voluntarily, while in sound and sober senses.

By the Lord Justice-Clerk.—There was certainly a slightly nervous appearance, but it did not seem to me more than might naturally be expected in a gentleman in his position, brought up on so serious a charge. There was some nervous excitement. He had the appearance of a man just out of a great and recent debauch, but quite sober. Witness was slightly personally acquainted with him. He was surgeon to the jail at Montrose; but he did not think he had spoken to him for more than a year. There was a decided difference in his appearance that day from his nervous excitement, from what he had seen before. Cannot say in other respects there was any marked change in his appearance; but he only knew him slightly.

Alexander Warden, clerk to Sheriff-clerk at Forfar.—Witness was present at the declaration of 2d October. Freely and voluntarily emitted, in sound and sober senses.

Declaration read as follows:—

Copy of Declaration of George Lillie Smith.

At Forfar, the 2d day of October 1854.

In presence of Charles Dickson, Esq., Advocate, Sheriff-Substitute of Forfarshire,

Appeared George Lillie Smith, who, being judicially admonished and examined, declares as follows, viz.:—Being asked how old I am, I declare that I decline to answer that, or any other question that may be put to me. And having heard this declaration read over, I decline to sign the same.

(Signed) Charles Dickson.
Henry Forsyth.
J. M. Milne.
A. Warden.
Michael Hinchy.
John Stewart.

Dr Smith declined to answer any questions whatsoever, or to sign the declaration.

Campbell's declaration admitted, and read as follows:—Appeared Robert Campbell, who, being judicially admonished and examined, declares as follows, viz., I am 69 years of age, and married, I was at one time a seaman, but have been unable for duty for some years. I live in High Street of Montrose; for the last two years I have had the charge of a pleasure boat, belonging to Dr George Lillie Smith of Montrose—I was out with the boat at Arbroath, and met Dr Smith and some of his friends there on Friday, 29th September last, and I took it back to Montrose on the afternoon of the following day. On said Saturday evening, after my return to Montrose, I met Dr Smith on the street, and he asked what was the matter with me, and I said I was a little the worse of grog; he invited me to go to the White Horse Inn, and get a beef-steak and a cup of tea, and I did so; when there he told me he was going to Noranside, and asked me whether I would go the length of Brechin with him, and get a ride in the gig. From what he said, I understood he wanted me to bring back the gig from Brechin to Montrose. I agreed to go, and he appointed me to meet him at the west side of the Tayoch toll-bar at ten o'clock at night. He then left me; after tea I went home to my own house, and remained there for about half-an-hour, after this I went to the shop of Alexander Mill, perfumer, and purchased a large walking stick for sixpence; I had got a loan of a

stick some time before from Mr Rial, at Lower Mills of Kinnacher, and lost it, and I intended to give him the one I bought in its place. I left Montrose about half-past nine o'clock that night, and I took said stick with me,—I walked out the turnpike road leading to Brechin, and when a little to the west of Tayoch toll-bar, and as the ten o'clock bell was ringing in Montrose, Dr Smith came up to me in a gig; he stopped and took me in, and drove off towards Brechin, nothing particular passed by the way, but Dr Smith was using threatening language towards Mr Smith of Haughs of Kinnaird. When near the prison of Brechin, he stopped and let me out, and he drove on to Mrs M'Bain's inn, which is almost close by. I walked on a little till he joined me; he put up the gig at Mrs M'Bain's. After joining me he asked me to take a walk a bit, and I thought his object was to put off the time until the horse got its corn, and agreed to go with him; he went to the door of an inn near by, and was to give me a glass of grog, but it was past 11 o'clock, and we did not gain admittance. We then went down towards the bridge of Brechin, and when upon the bridge he commenced sharpening a sword, which I then, for the first time, observed he had in his hand. He then took hold of me by the arm, and led me on having the sword in his other hand. We went along a dark road, having trees on both sides, and when along it a good way, he told me he was going to the Haughs of Kinnaird to set fire to the corn-yard, and then go round to the house and shoot Mr Smith if he should come out, He would not let me go, and would not return, although I entreated him. I was afraid for my life, and went on to the Haughs with him. When we reached the field at the Haughs, in which the stackyard is situated, Dr Smith took the stick from me, and cut it on a paling with the sword, and then broke it, and threw part of it away. He gave me the other part, and said it would be handier now for defending myself. Before this he had told me he had a pair of loaded pistols in his pocket. We went through the field to the corn-yard, and Dr Smith said to me "There's the stacks." He then went away from me, as I supposed towards the stacks, and in a minute afterwards I saw a bluish light and the stacks in flames. I was about 30 yards from him at this time. I then ran down towards the river side. I do not know what became of Dr Smith. After I got to the river side I threw the piece of stick from me into the river, near the mouth of the Pow.. I went down the river side till I reached the Bridge of Dun road, and I then crossed over the fields and entered Montrose by the Chain Bridge. A man opened the gate for me, and I gave him a penny to get through. I saw no man on horseback by the way. I reached Montrose at three o'clock on the Sunday morning. That hour struck as I was crossing the bridge. Dr Smith told me on our way to the Haughs, that he had sent his housekeeper for turpentine, and that he had some rags soaked with it in his pocket. He asked if I would put one of them into a stack, but I refused to do so. I did not know what I was doing, and mentioned the circumstance to no one. Dr Smith said he was to give me a present of his boat, as he was to leave the country. It was for my former services he was to do so, and not for accompanying him to the Haughs. I never told any person that I had set fire to the stacks. Being shown a piece of stick, I declare it is part of the one I bought, and carried, as before mentioned, and threw into the river. All which I declare to be truth, and I have signed the label at the piece of stick, as relative hereto.

(Signed) ROBERT CAMPBELL, &c., &c.

DEFENCE.

George Smart, corn-merchant, Montrose.—Has known Dr Smith eighteen years. He had a good practice; attended his family, and was much esteemed as a medical man. He went to London, with Colonel Fraser, who was insane—Dr Smith went as his medical attendant. This was, witness thinks, in November 1851. Colonel Fraser died in London soon after. Witness saw Dr Smith in London after Colonel Fraser's death, and he seemed agitated and afflicted by this sudden death, and seemed excited. He told witness he had not had his clothes off for several nights, during which Colonel Fraser had twice attempted his life. This seemed to affect Dr Smith so that he could not sleep.

Has observed that he was much more irritable since then. Never saw him take too much drink. Witness is aware he was in bad health in 1853, when Dr Christison attended him and Dr Booth, who was then in Montrose, but is now in active service as navy surgeon. Dr Smith repeatedly told witness that the disease was in the back of his head. That it was his mind which was diseased; if they could cure that, they could cure his body in forty-eight hours. This was in summer and autumn 1853. At times he observed very great excitement, *quite outrageous*, and threatening to take the life of different parties. Witness believes these threats were for imaginary injuries. Dr Smith told him the causes of complaint, which witness believes to be imaginary. He proposed challenges to one or two, which witness reasoned him out of. Dr Smith conducted himself in a reckless, extravagant manner at that time. Witness has seen him in his own house throw down his dirk, threatening that he would run people through the body, or cut them open. Has seen him after those excitements spitting mouthfuls of blood. These fits of excitement appeared to come on suddenly. Witness believed him perfectly sober at these times. He came repeatedly to witness's house, both in the morning, and evening, at that period, and sometimes staid all night, because he said he was quite unhappy at home. When he staid all night witness believes he wandered about the house a good deal during the night, as he said he could not sleep. He appeared to witness sober on these occasions. Often complained to witness of great pain in his head. On one occasion complained of his chest, and witness remained with him from seven evening to one in the morning. After he had had a hot bath, he told witness, next day, if witness had not put him into the bath, he was satisfied he would have been dead in fifteen minutes, as the pain was so great he could not have stood it. His eyes looked almost starting from his head on that occasion.

Could not say if he had had drink, but he did not appear to witness the worse of drink. He told me on one occasion, he had often lain all night in his own garden, and waked in the morning with his hair frozen to the grass. This was on an occasion in 1853, when he had gone out and lain on the grass in witness' garden, often having only a few glasses of wine with water. In the end of 1853, witness knows he went to reside at Haughs; witness called twice, once when he was in bed, and on the second occasion he was so unwell he could scarcely walk across the floor. He was restless and excited, complaining of his head. Occasionally witness saw him in Montrose after this—he observed his manner was entirely changed during the last eighteen or twenty months. He saw Dr Smith in Edinburgh in beginning of August 1854, ordered dinner for him at his hotel though he could not dine with him as going out. Witness went to dress, and when he returned found Dr Smith very excited at the way the fish was boiled, and would take no more dinner. This was in the North British Hotel—found him walking about the room swearing and very violent at the people, because he said they could not boil fish. Witness saw nothing to occasion this, he had only had a little beer. The fish was off the table, witness did not see it; witness wished him to have something else, but he would not. He walked out with witness to Waterloo Place where we parted. Witness was very well acquainted with his wife and family—his wife has been for sometime at Liverpool with her father. There was not the slightest ground for any imputation on witness with regard to his wife, and never heard the slightest blame attached to her in any way whatever. He saw Dr Smith only three or four times after August—saw him at Forfar after he was apprehended. He was dull the first time, less so the second.

By the Lord Justice-Clerk.—Dr Smith has attended very little to business during the last eighteen months. He took a partner in July 1853. He had spoken to witness several times on the subject, and he said by all means he should get a partner as he thought he was unfit to attend to business himself, both as unsettled, and on account of bodily health. This was in July 1853. He gradually got worse. Q. Asked the sort of injuries he stated against others. A. Captain Bouverie had employed another medical man to visit a patient, whom Dr Smith had not visited for ten days—this was in June or July 1853. He took this as a personal affront, and threatened to destroy Captain Bouverie and Lieutenant Black who lived with him. It was the same with regard to

Mr Boyd, the banker, who had called in another medical attendant in summer 1853. Dr Smith said he would destroy Mr Boyd if he could get hold of him, and wished witness to carry a challenge to him, which he declined to do. He thought Dr Browne of Dumfries had written letters in a way he should not have done, and threatened to destroy him also if he could see him. Recurred repeatedly to those offences in these people. Although quiet, the mention of one of these names would put him into a fury. Witness has seen him handling guns in his room, when threatening he would shoot people. Never saw pistols. The guns were duck-shooting guns. Witness never spoke to any of these gentlemen on the subject, except to Captain Bouverie and Mr Boyd; told them of his excitement, but did not think he would go out of his way to do them injury. Indeed, he could not, as his bodily health was so bad at the time—he could not go out. Captain Bouverie called on him, and he was quite satisfied on explanation that Captain Bouverie had done him no injury. Witness is not sure if there was a letter. He was not altogether satisfied, but was not so violent against him afterwards. Witness never heard of the insinuation about himself till within the last few weeks. There was no change of manner to him, witness, who had been one of his most intimate friends all along. Witness' nephew had not told him of the insinuation; believes it was entirely his own wish that his wife should leave him. Witness does not think there was sufficient cause for sending Mrs Smith to her father, and that Dr Smith should have paid more attention to her. Witness thinks Dr Smith had no grounds against her as regarded chastity, but that he had grounds for dissatisfaction with her. He did not attribute want of chastity to her, but other habits. He had five children; they were all left at home when Mrs Smith left; three remained at home, and two were sent to Jedburgh to school. He was remarkably kind to his children. Witness does not know how he got money. He was very little at home after midsummer 1853, and less after January 1854. When he stated he had lain out in the garden all night, witness believed it was a fact. He had told witness when in a quiet mood, and he was very truthful. He was generally suspicious; not so originally; he imagined the public were set against him latterly. He told witness he lived away for his health, and that he could not come near his own house, and that the public had been set against him. Witness has occasionally heard him talk of the Smiths of Haughs, but at that time he said nothing against them; latterly, he complained of Mr Smith as a bad man, a vulgar man, and so forth—calling him a blackguard, in short. Witness believes Mr Smith had shown him much kindness. Q. Up to the time you were in the habit of seeing him in the end of 1853, did you consider his reason overthrown? A. I did. He was incapable, witness thought, of judging right where his feelings were affected. Witness thought at that time he should have been sent to an asylum; believes this was also Dr Officer's opinion; he has said so to Dr Booth and to Dr Officer. If he could have interfered, he would certainly have taken medical advice about putting him in confinement. It was about four o'clock witness offered him dinner in the hotel; this was in the beginning of August. He told me he had come from Laverock Bank. He said that as the afternoon was wet, he daresayed he would rather take dinner than go there. He had not been in the yeomanry. Witness took no measures to have weapons removed from him. Had no right to interfere. He more than once asked witness to take a challenge to some person. He asked Mr Müller, a German gentlemen, to carry a challenge to some person he believes—Witness' opinion, known so far back as 1853 to Drs Booth and Officer, and to his family at home. He never heard him use threats against Mr Smith, but he was out of the country for some time after August 1853. He never saw symptoms of people plainly shunning Dr Smith; he had been much respected, and forbearance would have been shown to him.

By Court.—On suggestion of the Lord-Advocate—Dr Smith gave as reason for taking a partner the state of his health, in consequence of which he was not able to attend to his patients. Witness concurred cordially, as he thought that his mind was unsettled also.

By the Lord Justice-Clerk.—He told witness his illness was inflammation at the base of the brain. He never spoke to witness of his liver, except once or twice, when he said that was not the cause of his illness so much as his head, and that the doctors were mistaken about that, as the liver could be easily cured. When he said if the mind could be cured the body would in forty-eight hours; witness understood him to mean if the disease of his brain could be cured. He is satisfied Dr Smith was under the full impression he had disease of the brain, and this depressed him. Has heard him speak of this before both Dr Booth and Dr Officer. Witness believes they treated him for that.

William Jamieson, merchant in Montrose, and at one time Provost.—Witness knew Dr Smith a number of years—all the time he was in Montrose. He attended in his family eight or ten years. He had an extensive practice. Witness had constant occasions as provost to see Dr Smith in his capacity of a surgeon in a public situation. At that time he was highly respected and valued. There was nothing that witness could remark by way of excitement at that time. He has observed a change; should say within these two years, particularly in his conversation, manners, and dress. At first he was gentlemanlike: a man of refined feeling, and much delicacy in conversation—clean and neat in his dress. In the last two years in all these points he was changed. Loose in conversation, slovenly in dress, to a disgraceful degree. Witness never saw him otherwise than sober. He is aware he went to London with Colonel Fraser. Witness got into the same railway carriage with Dr Smith accidentally when returning. He thinks that is about three years ago. He was very much struck with the change in him at that time. His appearance was slovenly and dress dirty. Witness remarked to him that he appeared to be off a journey. Dr Smith said he had not had his clothes off or been in bed some eight or ten days, since he went to London. His manner was excited. Witness did not speak of the occurrence at all. He had confidence in him as a medical man, and others had so too. He does not think there was any prejudice in the public mind against Dr Smith. Witness has not seen him in his own house during the last year. In November 1853, he attended him for a week for rheumatism and cold. He remembers Dr Smith came to him one forenoon, about this time, very angry; he swore a great deal, and said he was determined to shoot Mr Boyd, the banker, and that he was on his way to desire Dr Booth to carry a challenge to him. After hearing him, witness said, "You talk like a madman; do you know there is law for others as well as yourself. I should say, as I always have, that you are a clever man, but you conduct yourself like a fool to talk in the way you are doing." Saw he was much softened by what witness had said. He walked about the room, whistled, and scarcely spoke for several minutes. Then sat down by the bedside quite quiet, talked of other subjects quite rationally for a considerable time, and then went away. Witness did not think him under the influence of drink. He had the idea that Mr Boyd had interfered to get his appointment as surgeon at the factory given to another. Witness was not provost at the time referred to. This was the last time he attended witness. Dr Smith prescribed for him quite correctly. He had seen slight symptoms of a similar kind before, but nothing so strong as this. His manner was very violent and excited on that occasion. He had a very trifling habit of swearing before the change alluded to, just as many men would; but latterly he was greatly addicted to it.

By the Lord Advocate.—It was for violent rheumatism and influenza he attended witness, and attack in the throat; he was quite satisfied with Dr Smith's skill and professional attendance at that time. He has not had occasion to consult a medical man since.

By the Court.—Is aware Dr Smith had much given up attending patients and left it latterly to Dr Officer.

By the Lord Advocate.—Q. Did you see anything after the time referred to, to alter your opinion that he was a sensible and intelligent man? A. I have seen very little of him since; he was very little in Montrose.

Peter Matthews, railway guard on Scottish Central Line.—Remembers a collision on the line in April 1854; train from Perth to Edinburgh, about two miles to the south of Stirling, between Stirling and Bannockburn, by an engine coming up behind. The last carriage was much injured. There were two passengers in the centre compartment. Dr Smith was one of them. Witness saw him rising as if from his knees after he was cast down. He appeared almost insensible. Complained of the shock afterwards. Witness saw him afterwards sitting at the foot of the embankment, and asked him to rejoin. He was not quite recovered. Seemed confused and unwilling to rejoin the train. Witness led him to it and he got in. He seemed hurt; does not remember any blood. Saw him sometime afterwards. Cannot say if two months. He recollected me quite well, and talked quite rationally and collectedly. Witness asked him to take his seat; he looked at him and hoped they would get along more safely than last time.

James Wilkie.—Remembers a collision. Dr Smith suffered a little from it. Witness saw a little blood from his nose, and thinks, from his ear.

James M'Gregor, keeper of the National Hotel, Register Street, Edinburgh, knows Dr Smith, who lived in his hotel the latter part of August last. Came on Tuesday 22d; remained till the morning of Wednesday 30th. After the first two days witness saw a good deal of him. Occasionally noticed him very excited, at other times melancholy, and at other times very merry. The gentlemen in the public-room said they did not consider it safe to be alone with him. He carried a knife about with him—it was a sort of dirk, in a sheath, like that now shewn. He frequently exhibited it in the public-room. He used to lay it on the table beside him when taking food. He said he got it from his father, who desired him to take care of it, for it had done deeds before, and he indicated it might again. This was complained of by the other guests, and witness took the knife under his own charge. Dr Smith remained three to four days after this. He was quite pleased witness should keep it, and desired him not to lose it. Witness told him the other guests did not like it, and therefore it was better he should take care of it. He said, by all means. He had not used it at meals. After witness took possession of it, he saw Dr Smith going out of the hotel with one of his large carving knives in his outside coat pocket. He got it away from him without Dr Smith noticing it. Dr Smith has told him that he was haunted or hunted by spies looking after him. Did not say who, or the cause, or where they came from. He said they stopped when he stopped, and turned round after him; he had warned them against such conduct. He seemed to wish something to protect him when he went out. In course of that week witness had some conversation with him as to some person on whom he wished revenge. He mentioned no name, nor did he particularly mention the cause. The person was supposed to be in Edinburgh. He asked witness in reference to this if he would go out and second him in a duel. He refused. He then asked him to get another to do this. Witness promised that, in order to please him for the time. He named nobody—just said that witness would find a second for him. He mentioned that some person had injured him, or his family. No name mentioned. Something about a person having seduced, or attempted to seduce, his wife. He did not speak very loudly, but was excited; in the public-room, before others, he wished all who came in to partake of whatever he was partaking of, and was excited when they refused. On one occasion saw him in tears; it was on a Sunday. He had taken one of the witness' sister's children on his knee just before this. Sometimes laughed very heartily, and when music was passing, would lift the window and throw out money to the musicians. The simplest cause in the world would sometimes make him laugh. He sometimes went in trousers, and a sort of flannel shirt he wore, to the front door. He left about two A.M. of Wednesday morning. Had seen him the previous evening, when he went out, with somebody. Returned about half-past one. Witness let him in. Dr Smith wished him to go to his bed-room and bring down his plaid, that they might go to the Calton Hill and sleep out all night. Witness remonstrated, and Dr Smith said if he would not go with him he would stand where he was on the steps of the door all night. Witness shut the door on him. He rung violently, and said he was much insulted—ordered his luggage to be brought. Witness called the waiter to assist him; then he wanted his knife, which he immediately got. He was in great excitement at this time, and made a great noise after witness came down again, threatening he would take witness' heart out with his knife. Walked backwards and forwards in the lobby, rubbed the knife on the steps, and put it between his teeth. Witness had locked himself into a room, from apprehension of what he might do. Had paid his bill the forenoon of previous day. He left in a cab about two o'clock A.M. While he lived in witness' house he could take his glass, but he never saw him the worse of it. On the occasion last referred to thought him in one of his excited states, and not from drink.

By the Lord Advocate.—Witness cannot give a notion of how much drink he had in a day while in the hotel. He came back in about a fortnight and apologized—said he was sorry that he had been in one of the excited fits which he was in the habit of taking. On this last occasion he was perfectly quiet and rational. He went out pretty early generally when at the hotel. Witness does not know where he was living before he came to his house; when he left he went to the Waterloo Hotel.

By the Lord Justice-Clerk.—Dr Smith ordered great quantities of drink for other people, and cannot say how much he took of it himself. He did nothing to induce witness to notice that he was taking too much drink. He was not so much complained of by others, as to his drinking too much, as to his not being in a fit state to be there. The other guests did not complain at all to witness of Dr Smith's drinking. When he came in at eleven he wished to have supper for himself and a friend; but witness could not admit his friend at that hour. He (Dr Smith) said they would go to Rampling's in the Waterloo, and get what they wanted. Witness does not know what he had when away, but did not ascribe his state when he returned to drink.

Thomas M'Lean, waiter in M'Gregor's National Hotel, Register Street.—Witness was there the end of last August, when Dr Smith was there about a week. His conduct was very remarkable. It struck witness that he was not altogether right in his mind at times. Gentlemen in the house got themselves locked into their bed-rooms to be out of his way. Sometimes he was in great excitement; at other times mild. He wanted a Norwegian gentleman who had just supped, to sup with him; he declined, but did at witness' request to please the Doctor. Immediately after he got up, and went to his bed-room. Dr Smith had sworn he would do for him, if he did not remain. He carried a knife in a sheath, and said his father had done something with it. He thought people were watching him, and said he would do for them. He mentioned Dr Alexander of Edinburgh, particularly, as watching him. He said some person passed and repassed the hotel daily, watching him. Witness had seen this person often passing, and said he was not watching Dr Smith. He wanted a person to come to see his pistols properly loaded, that he might fight a duel with Dr Alexander. Witness saw no pistols. Mr M'Gregor got hold of his knife, and he was much displeased at this, and got a carving knife, which Mr M'Gregor took out of his pocket. Sometimes merry, and suddenly he would become sad. He was in steady habits himself while he was in the house, but ready to treat others with what drink they wanted. Offered no meat to any one that witness knows of, except to the Norwegian. Witness was roused to assist him in packing his luggage the morning he left. He was in a great state of excitement. Heard him sharpening his knife on the stair, and threatening master's life. Dr Alexander lives in Queensferry Street. Never heard Dr Smith say where he lived.

Professor Christison examined by the Dean.—Witness was sent for in September 1853 to visit Dr Smith—he knew something of him previously, first as a student at the University, afterwards but seldom had met him in practice. Partly from what he had seen, and partly from what he had been informed of, he always considered Dr Smith as a very intelligent practitioner. In September 1853, found him at Noranside, and went with Dr Smith's partner, Dr Officer, and Dr Steel of Forfar. Made a careful and thorough examination of the patient, and the result was that he laboured under undue enlargement of the liver, great disturbance of the circulation, very rapid pulse, 120 to 130. There was also considerable mental excitement. He was always an excitable man. Witness ascribed the excitement to explanations given him at the time—that it was caused by what had occurred in his domestic life, and by the probability of that difficulty being arranged at that time, but still uncertain. Dr Smith complained of heat in the head, uneasy sensation in the back of the head, and want of sleep. The condition of his health was such that mental excitement was likely to produce great effect on him. Witness formed a very clear opinion. 1*st*, That he was very dangerously ill, as regarded the liver. 2*d*, There was great risk of his passing into a state of insanity from the probable production of cerebral disease. Does not mean that there was cerebral disease there, but that he thought it was forming. Thought this probable, because symptoms of disease of the head forming, and risk from his disposition of being exposed to constant sources of excitement. The two other medical gentlemen agreed with me in this opinion. Witness left Edinburgh in the morning, and returned at night. Cannot say how long he was there—it would be some hours. Did not see him again till towards the close of July last year, when he saw him at Miss Carnegie's, Laverock Bank. This was incidentally, but did not examine him professionally. Witness had been asked to see Mr Carnegie. At this time Dr Smith was much better as regarded the enlargement of the liver. His general health was much improved. He was still an excitable man; but the state of excitement was gone. He was in a perfectly calm and quiet state at that time. Does not recollect that he was informed that he had had a serious attack shortly before. He entertained more hopes of recovery then than at Noranside. His expressions and conversation shewed great diminution of the excitement formerly seen, although he obviously still retained an excitable temperament. Witness has a strong impression he saw Dr Smith not long after in witness' own house, and had a short conversation with him when he seemed quiet in manner, as at Trinity. Did not see him again till last Sunday forenoon, in the prison of Edinburgh. Found him very poorly in bodily health. There is still a large tumour, not exactly in the same situation, but, from what witness ascertained formerly, still attributable to the liver. His expression was that of an excited person, though calm and quiet—very much that of a person recently intemperate, so that he asked if he had been getting stimulants, and the governor said he was not, nothing but 1½d. beer. Witness, therefore, ascribed his appearance to physical disease. He has not seen Dr Smith at any time in a state of insanity. Considering all that witness knows of his case, should think it very probable he may be occasionally in a state of insanity. There is nothing inconsistent with this, in the calm, quiet state in which he found him. By accident, a person conversing with him in prison might discover symptoms of insanity, but witness thinks it highly probable he would find none. He saw none. He thought Dr Smith's mind weakened, but saw no insanity. He came upon subjects which might have caused excitement, but found none. We know that unreasonable suspicions, and a disposition to gratify resentment, are common in cases of insanity induced by ill health and mental depression. Witness supposes he suspected his most intimate friend of too great intimacy with his wife, and yet continued the friendship; he thought he had pistols when he had none, and the like. Witness would consider these strong presumptions of insanity, and would consider the man who was under such delusions insane for the time.

By the Lord Advocate.—On none of the four occasions when witness saw Dr Smith did he see any evidence of insanity. He conversed rationally and intelligently as far as he could judge, but he did not know whether what he stated was correct or not, and only suspected delusions. He told witness two men and a woman had assaulted him on the streets, after saying they knew him. This was at some time, witness does not know the date, when Mr Smith of Haughs of Kinnaird was living in Edinburgh. This was one of the most striking. On none of the previous occasions was there anything savouring of delusion—nothing to shew he did not know right from wrong, as far as witness saw. If cerebral disease did not form, would not expect insanity. The view witness took was, that this disease would probably form, and lead to insanity. He cannot pronounce with certainty that there is cerebral congestion, but he labours under a strong impression that there is, and has been for some time, a cerebral affection or a more permanent disease of the brain. He deduces this opinion from excessive restlessness at night, extreme frequency of pulse, which is generally 120—his complaining of heat in the head and confusion, his description of the illness he had during the night in Forfar jail, when he thinks he fell several times on the floor—the partial paralysis which witness understands he laboured under—general state and appearance of the eye—are the grounds he goes upon in thinking that he has congestion, or some other more serious disease of the brain. These do not necessarily indicate congestion or other disease of the brain, yet so much so that witness would treat him on that footing if he were his patient. Supposing he was labouring under congestion of the brain, this is not inconsistent with perfect sanity. It is the character of such persons to shew very readily indications of insanity to persons beneath them in station, or with whom they are familiar, but to keep it out of sight of those who are in the same station with themselves, or above them, or not familiar with them.

Q. Do you think the surgeon of the jail seeing him twice a-week, or you, had the best opportunities of judging of his case? A. I think the surgeon of the jail visiting him frequently, and conversing with him, should be well able to judge. It is impossible to say how an accusation of crime would act; sometimes it acts one way and sometimes another. Undoubtedly the accusation of such a crime is as much a source of mental disturbance as domestic disquiet; yet witness has known it otherwise. Dr Smith's composure on the present occasion does not surprise him as a physician, although he were told he was insane. The disturbing causes are not so constantly under his observation in jail as when living at large—he is away from scenes and persons to suggest these ideas,—away, too, from all stimulants, living a perfectly regular life, and seeing very few people. Persons are put into asylums to get rid of such causes of excitement.

Re-examined by the Dean.—So far as witness goes upon anything except his own knowledge, it is by carefully ascertaining facts, without which medical evidence could not stand.

By the Lord Justice-Clerk.—When witness said he appeared of intemperate habits, he meant Dr Smith had the appearance of a person who had been drinking the previous night. Not in speech, but in his eye and appearance. In September 1853, witness was made aware that Dr Smith used spirituous liquors to a greater extent than persons generally, but saw nothing to shew he was a man of intemperate habits. Q. Supposing what you apprehended had gone on, and he had become insane at times, so as to commit a criminal act on a Saturday night, would it surprise you to find him cool and quiet on Monday and Tuesday? A. No, not at all surprising, from what we know of the history of cases of the kind. It is not uncommon, after indulging diseased excitement, to find prostration; the system is apparently exhausted by excitement. Q. If accustomed to spirits previously, do you think it likely he would refuse stimulants if offered to him? A. Sometimes disgust ensues, and the person will refuse: the general rule is rather the reverse. When he told me of the incident as happening on the street, saying that never since he was a lad had he been engaged in a personal strife except once, and then mentioned what had occurred quite naturally, as one would mention an incident of one's life, there was no appearance of feigning delusions to mislead witness. On the contrary, he laboured with great success to give a different explanation of the threats against M'Gregor of the National Hotel, by giving a jocular account of them. The paralysis must have depended on some affection of the brain, and most probably on congestion. He told witness nothing of a duel with any person.

Donald Mackay, governor of Forfar prison.—Dr Smith was sent to the prison of Forfar 2d October, and remained till 11th of this month (January)—three months and ten days. His health was bad all the time. On getting the care of prisoner, witness was led to believe he was a man at the height of delirium tremens; and from his experience, and with the approbation of the surgeon, he was prepared to treat him as other patients of that kind are treated. Tried to find out his habits, so that he might give him about as much as he was accustomed to, and wean him gradually. He accepted a glass of punch, and took it slowly. As experience went on, gave him a similar quantity three or four times. Witness means a common glass of whisky made into punch each time. On one occasion he asked for a little spirits, as he thought something had disagreed with him. Witness gave it to him. He shewed no desire for ardent spirits. He varied in his state, particularly the first ten or twelve days; generally quiet, except when excited, when he would go into a paroxysm, from a letter or a friend not coming when expected; on which occasions witness would be glad to leave him, and on his return, would find him prostrated and low. In these excited states he was extremely violent. Witness cannot say he was much afraid of him, but sometimes would rather not have been in the cell with him. He has seen Dr Smith in such a state of excitement that he can imagine nothing too violent for him to do at that time. Remembers going into his cell one afternoon and finding him, seemingly, very dejected. Entered into conversation with him; in course of a short time his face and head became livid. He staggered round to the fireside and put out a large mouthful of dark-coloured blood, and then returned to his seat. He said don't be alarmed, this will soon wear off. He became as usual in about a quarter of an hour, and his face of the usual colour. On another occasion witness remembers his having a fit in the cell. He went in between eleven and twelve; heard some shuffling in the cell—went in, and found him on the floor, with his head on the wooden bed. Witness thought he was in a fit; he was recovering when he went in. He did not perhaps answer me immediately, but after a time he asked witness to take him to the passage for air or change. He did so, and Dr Smith walked about there a little, after which he went to bed again and witness left him. On going into the cell he has found him occasionally in a state of idiotic laughter, with no apparent cause. Does not think he asked him the reason. Does not think at that moment he could give a rational account of it. These variations from one state to another were more sudden than frequent. As hospital sergeant, witness has had occasion to see many patients under delirium tremens. His eye made an impression on witness from the first that he was not right in his mind; watched him, consequently, for the first ten or twelve days, and met with some slight confirmations of it; such as putting the same question repeatedly after it was answered, and being so abstracted that it required a great effort to draw his attention. After the first ten or twelve days witness considered him better, he became easier in mind; until he became aware insanity was to be pleaded. After that he became extremely excited, restless, and slept none, being very ill-pleased at the proposition. After this witness saw more fits of excitement than before. Witness has no idea he was feigning insanity; the reverse is his impression.

Cross-examined by the Lord Advocate.—This change was some weeks ago, and before the beginning of this year. Witness has expressed to the doctor of the jail that he had observed something in the man. Did not tell him all the facts now mentioned, as he had by that time formed his opinion. Many of these facts are to be found in his journal. It is witness' interest and duty to report to the doctor to relieve himself from responsibility. Witness has seen him often excited subsequently to first ten or twelve days, and before he knew insanity to be pleaded. The doctor of the prison was never present when he was excited. He visited him under the best circumstances. At least twice a-week he saw him—he should rather say oftener.

Re-examined by the Dean.—Witness means by visiting him under the most favourable circumstances, that when it was announced to Dr Smith that any gentleman was coming he would gather himself up, and it would require one who saw him often and knew him well, to imagine anything was wrong with him. He exerted himself in a way which surprised witness. He would do the same thing on Dr Malcolm's visits. Had witness seen no more of Dr Smith than these medical gentlemen did, he would not stand in this box as he does. He would not have supposed anything was the matter with him.

Dr William Browne, Medical Superintendent of the Asylum, Dumfries.—Has been engaged for about twenty years in attending cases of insanity. He was medical superintendent in Montrose Asylum in 1836, when Dr Smith came to Montrose. He was a personal friend of witness, and attended his family. He was a respectable and skilful practitioner. Witness left Montrose in 1838. He saw Dr Smith occasionally after that till 1847, and did not perceive any change during that period. After 1847 he saw less of him. Had no written correspondence with him then till January 1854, when he wrote to witness for his opinion as to the treatment of partial paralysis he was then under. After that time he sent witness such letters as he formerly would not have written. They were not like the letters of a man of education. Sometimes they were not intelligible—partly vague, partly confused. They contained suspicions of conspiracies and persecutions against himself—threats and denunciations against various individuals, and general marks of great excitement. He accused witness of uttering a calumny against him, for which there was no foundation, it was a mere imagination. Witness agreed to Dr Smith's coming to visit him in January 1854, but he did not come. He did not see him till very recently. Witness was prepared to find him changed. Had had correspondence with him up to the present time. His handwriting was altered, and there were mistakes in spelling. The handwriting was more ill-formed than formerly, and in some instances illegible. Cannot say it appeared as if the eye had not guided the hand correctly; thought rather in action of the hand, as if it had not power to form letters as formerly. The bad spelling was new. Dr Smith was a man of education. Witness was acquainted with his constitution generally and temperament. These letters gave him apprehensions connecting them with such knowledge—he apprehended some form of mental disease. He visited him last month in prison as a friend. His aspect was changed; he should not have recognized him. The expression of the eye, the features generally, gave indications of disease. The eye was bloodshot, wild, the white part tinged with yellow and prominent. Expression of the countenance indicated violence. Found he had disease of the liver, heart, and some parts of the brain. Infers this last from the extreme rapidity of the pulse, alteration in mode and precision of articulation—pain on skull being struck slightly, or the jaw so struck, or the head behind. He described himself as suffering pain, weakness, &c., indicating diseased state of body, also sleeplessness; and the symptoms he detailed were much in accordance with what witness observed. Witness thought the disease of the brain, in all probability, the result of inflammation or structural alteration consequent on the symptoms of paralysis. He thinks he could form this opinion with some confidence; he would have treated him correctly on the supposition that it was. He wishes to mention two symptoms, 1st, the face assuming suddenly a livid colour, followed by vomiting dark-coloured blood, and staggering across the room. 2d, He was found once in the cell, apparently recovering from a fit. He desired to be taken into the passage, where he gradually recovered. Q. Are these confirmations of your opinion, or the contrary? A. Decided confirmations. Witness took some trouble to ascertain his mental state, and found great excitement. Prisoner's opinions are not such as they were formerly—abolition of notions of right and wrong, violent antipathies and suspicions, repetitions of what he had written to witness. So violent were his bursts of passion, that if not familiar with such paroxysms, one might have felt some alarm for personal safety. He spoke of the charge against himself, although witness had said he did not wish to hear of it. He said, supposing he had done it, that he treated it as a joke. At another time, he spoke of it as an act of righteous vengeance. He did not say he had committed the act; but told witness he had been in the house of Mr and Mrs Smith, and deposited powder under the head or bed of Mrs Smith, which he connected with a train outside, by means of which he could have destroyed her. He did not say why he refrained from doing it. Witness did not believe him—he regarded it as an insane delusion. Inferred, or else Dr Smith told him, his revenge was connected with some persecutions against him. Dr Smith vented threats in his presence against persons in general terms as *they* and *them*. He did mention some names Mr and Mrs Smith, and witness thinks Dr Officer, not Mr Smart. Witness paid him two visits in one day, some hours each time, and had ample opportunities to form an opinion in connection with prior knowledge. He recognised witness certainly, and seemed disposed to speak with more candour, and more freely than witness deemed expedient. The extreme excitement was during the second visit. Witness attributed the freedom with which Dr Smith spoke to the excitement. He attributed the excitement to disease of the brain. Witness has no hesitation in saying Dr Smith was insane on the second visit; he was incapable at that time of exercising reason, which was either asleep or overpowered by passion. Did not think he was then capable of distinguishing right from wrong. Witness should not consider him a responsible agent during part of that time. At times he was perfectly calm and collected. Would expect such alternations in that disease. A stranger visiting him when calm and quiet, might discern nothing wrong, if he put out of view the physical disease. It is common for such persons before strangers, or for specific purposes, to observe calmness of demeanour. It is one of the symptoms of insanity to deny it. Witness thinks Dr Smith capable of so controlling himself, as to lead a stranger to think him sane if certain topics were avoided. In all probability the introduction of the story about the Smiths would have excited him at once.

Cross-examined by the Lord-Advocate.—Do you think Dr Smith could have given an account of what he has done during the last two months without betraying symptoms of insanity? A. I scarcely think he could. Q. Do you think these symptoms would have eluded a medical man attending him twice a week? A. I think not, if the medical man was familiar with mental diseases; he would have exhibited, or have been induced to exhibit them. I think, in conjunction with witness' previous examination of physical condition, in one visit I could have detected them. It was chiefly in my first visit that I detected traces of delusions. He spoke of matters in his own history, and he did not appear to me to take distinctions which persons capable of distinguishing right from wrong would have taken. Q. Did he seem to know himself liable to punishment for the crime? A. He seemed to think he was then undergoing punishment unjustly. I have no doubt he knew the fact that he was liable to be tried. On both visits I considered him insane, but on somewhat different grounds. During my first visit, he directed chiefly his observations to the change in his mental constitution; on the second, it was to the circumstance of his extreme violence, in consequence of my being a short time later than his appointment. I think allusion to the fire-raising more calculated to set him off than other subjects. Saw him to-day, and think he is still insane.

Re-examined by the Dean.—Mean his general conduct is that of insanity, manifested in lesser or greater degree.

By the Court.—In 1848 he made accusations in his letters. Afterwards he resumed correspondence, apparently on the same terms of kindness. When witness visited him in jail, he made no explanation or allusion to any cause of unkindness. There is variation from time to time in manifestation of insanity, usually where, from disease of the brain, greater than where there is no such organic disease. Disease of the brain may be going on, and nothing have occurred to indicate insanity. Witness was shewn a letter of 25th September, addressed by the prisoner to Mrs Smith, and read it, and said he would not have recognised it as a letter of his; his handwriting was very much altered, and such as he should say Dr Smith was incapable of writing, as regards its composition, when first he was acquainted with him. In a particular class of mental diseases, change in handwriting is the chief and commencing characteristic, and equally so a change in composition. In many cases, intimation of intention to take revenge is made before the act. It is constantly observed in cases of mania that, after an outbreak, comes a period of exhaustion and comparative quiet and apparent improvement, the ultimate result, probably, only being increase of disease. The largest proportion of cases of insanity is supposed to depend on physical causes. Great experience will, witness thinks, enable a man to form an opinion in such cases, when a man of equal or greater intelligence, without such experience, might fail. If witness' object is to discover the existence or non-existence of insanity, he alludes to subjects likely to excite manifestations of it. Dr Smith gave witness no intelligible account of his antipathy against the Smiths, but witness did not allude much to that. Witness alluded to his antipathy to others. He gave reasons, such as against Dr Officer, about their contract, which seemed to witness natural and justifiable. At the second visit Dr Smith's local agent was present. At the suggestion of the Lord-Advocate, witness was shewn a letter from Dr Smith to Miss Carnegie of 1st October 1854, and asked if the same alteration he had spoken to in the handwriting appeared on the address of that letter and in the letter itself. A. It resembles more closely his former writing; but still I do not think it is such a letter as he would formerly have written.

By the Lord Justice-Clerk.—He never dwelt to me on anything in regard to his wife.

Dr William Steel, surgeon, Forfar.—He is acquainted with Dr Smith. Witness was not personally acquainted with Dr Smith till he saw him professionally in the end of August or beginning of September 1853, when witness, Dr Christison, and Dr Robb came to joint opinion about his case. Saw him afterwards at Noranside on that occasion, and again in his own house in December. Witness found him then in a state of great excitement, exceedingly restless, wild, he would almost say, of a maniacal expression. Eye suffused; pulse very quick and pretty full; he was seriously ill. He was labouring under disease of the liver, and witness considered also under chronic inflammation of the membranes of the brain. Soon after he went to Haughs of Kinnaird, and was seriously ill there also; all the symptoms were then in a more aggravated form. Witness should say, on this second occasion, his mind was affected by his diseases; he said so at the time. Leeches were applied to the head, and he underwent a pretty severe course of calomel. Witness attributed his condition under that attack to brain disease. Witness was at Noranside 1st October last, visiting Mr Carnegie. Dr Smith was there before him; he saw him in the drawing-room, and was struck by his appearance. It was wild; his eyes red, and face flushed; his expression had very much the appearance of an insane person. Dirty and unshaved. He saw Dr Smith at dinner that day. He was quieter, but still unwashed and dressed as before; found him more excited after conversation with Dr Officer and Mr Smart. We concurred in the propriety of sending him to an asylum. Witness was decidedly of opinion and in favour of doing so. He considered him insane. Had no doubt about it.

By the Lord Advocate.—When witness saw Dr Smith with Dr Christison in September 1853, he had no doubt he was labouring under disease of the brain. At Haughs of Kinnaird he was told Dr Smith had taken more stimulants than proper; thinks Dr Officer was with witness then, and they made inquiries together. Witness ordered less, and stimulants to be changed; he allowed him five or six glasses of sherry within twenty-four hours. Witness was told he was getting several glasses of brandy with water previously. He only saw him once at Haughs of Kinnaird. Very great visible excitement, which any one in company must have perceived, while he was at dinner at Noranside. Witness drove him to Forfar; he calmed down exceedingly, and when he left him he thought him quite capable of taking care of himself. Witness knew he was going to Dumfries. He would allow nobody to go with him, which Dr Officer had proposed to do.

By the Lord Justice-Clerk.—No particular outbreak at table. Mr Carnegie remarked nothing particular. Witness believes Miss Carnegie was at pains to prevent him doing so.

Thomas Morrison, superintendent of Nottingham Lunatic Asylum, formerly of Montrose Asylum for two and a half years, 1849 to December 1853. Witness was acquainted with Dr Smith during greater part of time he was there. Recollects his being unwell, which he ascribed to domestic afflictions, and to attendance on Colonel Fraser, with whom he went to London. It was in 1853 that witness' attention was directed to his health. Previously he was a very strong man. The change was very marked. He told witness he was falling off, and he believed he was so. He became much more irritable, and upon very trivial causes. He was much more slovenly in dress and personal appearance. Previously he was not so at all. In 1853 observed an increase of his illness, and saw causes of domestic unhappiness when frequently in his house. Witness was afraid he would commit suicide, and hinted his suspicions to several persons that Smith was going out of his mind. In July 1853, Dr Smith travelled with witness to Aberdeen. His object was simply recreation and health. At the hotel in Aberdeen, he one moment was groaning on the sofa, and the next moment in boisterous mirth, so much so, that the waiter or the landlord asked witness if he were one of my patients from the Asylum. He could take no animal food without vomiting. Took some fruit and wine, which were the only things would remain on his stomach. It was Burgundy he drank, which he sent out for. Between two and three bottles of Burgundy were drunk between them during the day. Witness had his share. Wine did not affect Dr Smith. Witness never saw him tipsy in his life. The attacks he was subject to might have been increased by drinking, but witness does not think they were. Next day we took a drive into the country. He insisted on getting on the coach-box to drive, but did not retain the reins five minutes. He was very sleepless, and on both nights he slept at the inn he came into witness' bed-room during the night. He frequently had spoken to witness of sleeplessness previously. At home witness believes he was in the habit of going out at night. Witness has seen him in his own house lying asleep during the day. He travelled with Dr Smith twice to Edinburgh before they went to Aberdeen. Dr Smith slept on one occasion in the Waterloo Hotel where witness put up. On the other occasion he staid with Miss Carnegie at Laverock Bank. Witness made himself acquainted with his symptoms then and previously. Never found his pulse under 90, frequently 120. Eyes bloodshot, pain in region of stomach, swimming pains in the head, liver diseased, spots on his face, which bled at the slightest touch, and which it was difficult to stop, indicating bad state of circulation. Witness never examined his state otherwise than as a friend. Saw him on Sunday night; not previously since the end of 1853. He is much changed. Liver much increased in size. Memory slightly affected. He knew witness at once. Their conversation was chiefly about his present condition and past evils. He expressed great dislike to his partner. What witness observed confirmed the impression he previously formed that he was of unsound mind. He was with him about an hour and a half. He did not find him in an excited state, but pretty calm and quiet.

James Thomas Alexander, residing in Randolph Place, Edinburgh.—Witness became acquainted with Mrs Smith about April 1854, when she was in want of lodgings. Ascertained that she was the wife of Mr John Smith, Haughs of Kinnaird. Had occasion to visit her infant afterwards. Her husband came to Edinburgh, and she wished him to keep an arrangement that she was to stay away six months. In consequence, witness went with her to Laverock Bank. Mrs Smith went in to see Miss Carnegie, and witness found Dr Smith there, and had conversation with him outside. He was in great excitement, and abusive of Mr Smith. They left him at Laverock Bank. Witness had not allowed Mrs Smith and him to meet in consequence of what he understood about the separation. Witness saw Dr Smith again, and a third time he saw him walking with Miss Carnegie. He came up and took witness by the collar, and said if he did not mind his own business he (Dr Smith) would knock his brains out. Witness said he had given him no offence, and he went away. He had given no offence except preventing a meeting between him and Mrs Smith. Witness saw him again a considerable time afterwards, and he came up and threatened witness' life, and said it was lucky he had no weapon with him or he would have done it. Witness was greatly alarmed about him. Saw him afterwards sometimes walking about; not to speak to. Saw him once afterwards walking with Mrs Smith; he left her and followed witness down St Cuthbert's Lane, but did not speak to him. Witness never had any personal conflict with him. If he had any mark on his arm which he attributed to witness, it must have been a delusion. He never fought a duel with Dr Smith.

By the Lord Advocate.—Witness did not take hold of him when Dr Smith took him by the collar. He thought the least said soonest mended.

By the Lord Justice-Clerk.—The impression witness formed on both occasions decidedly was that his conduct proceeded from drink.

For Campbell.

Mrs Catherine Burn.—Her husband is a shipbuilder in Montrose, and resides near the beach. Witness has known Campbell upwards of twenty years. She never heard anything against him. He is a quiet and inoffensive man; very unlikely to do any injury to anybody. He was boatman to Dr Smith for a number of years. Witness recollects Dr Smith coming to her house on 26th September, saying the boat was aground and it would take the next three quarters of an hour till it should float. Dr Smith asked her for a sheet of paper about the time he was going away. (Witness was here shown a letter of 26th September by Dr Smith to Campbell.) It is the letter; Dr Smith gave it to her to read before he gave it to Campbell; he then handed it to Campbell. He told me to take care Campbell did not sell the boat, but give it to a friend or burn it. This was at eleven A.M.

James Auchnie, Montrose.—Has known Campbell all his life. Witness took charge of Dr Smith's boat at one time. Afterwards Campbell did. Campbell told me he had got a letter from the Kinlethmont people to meet them with the boat next morning. Campbell and witness went away about half-past eleven at night, and reached Arbroath about seven on Friday morning. Dr Smith joined them with the Kinlethmont people about ten; they went round the Bell Rock, and returned about four P.M. On Saturday Campbell and witness alone left about eleven A.M. to return, and reached Montrose between four and five. They had some spirits in the boat, and afterwards returned to Montrose. Campbell left witness' house about half-past five. He was the worse of drink at that time, and left the house to go home. Never saw him the worse of drink except on that occasion. He is much respected in Montrose. He is a harmless man, and never heard of his doing ill to anybody.

David Mackie, Provost of Montrose.—Thirty-five years there. Often saw Campbell. Always considered him a very decent, quiet man. He brought up his family respectably. He is about seventy years of age.

The Lord Advocate addressed the Jury for the Crown.—He said, as to Campbell, if there was ground for doubt if he was actually present at the commission of the offence, concurring and aiding therein, and if it rather appeared that he went but a certain way under the natural influence exercised by Dr Smith over an old servant, it was for the jury to consider whether they should not acquit him. With respect to Smith, after stating the evidence applicable to the facts of the crime actually committed, he referred particularly to the case of a lad of weak intellect tried two years ago by Lord Cockburn for cutting off a child's head and convicted, and to the case of Gibson tried by the Lord Justice-Clerk in 1844, on which occasion it was laid down that it was not sufficient that the defence of insanity should be raised on behalf of the prisoner, but that there must be established beyond doubt such insanity as to exempt him from punishment by evidence which brought complete conviction to the minds of the jury, who were to decide the question of insanity, and not the medical witnesses who might be called. In the case before them he said the question of fact for the jury was, "Did Smith, in committing the act, know it was punishable by law?" That this was the greatest and most important question he had ever known in a court of justice; that the like question would arise were the crime murder, and the issue to be capital punishment. He admitted that Dr Smith was of a very excitable and irritable disposition, but that it might be doubted if the evidence went beyond establishing that, and the effect on such a temperament of domestic grief, strong and irregular passions, and indulgence in stimulants. But suppose they were to go farther, and hold with Dr Brown that there were times when Smith's state of mind went beyond this, still the question remains, "Did he, in fact, know, when he set the stack-yard on fire, that he was doing what is contrary to, and punishable by, the law of the land. His Lordship then went over Dr Smith's history, showing him as a man of education and intelligence. He did not think much effect could be attributed to his journey to London with Colonel Fraser, for his ill health did not begin till 1853, considerably after that journey. It was owing to his ill health that he went into partnership with Officer—in that there was nothing uncommon. He then separated from his wife;—from this occurrence no insane delusion is to be presumed. On the contrary, one witness, George Smart, stated that there was some ground for his so separating, though he thought it (as often happens) not adequate. Suppose it proved that he had congestion of the brain, that neither necessarily leads to insanity, nor to incapacity for the ordinary enjoyment of life. The same thing may be said of disease of the liver. Dr Smith was left in the management of his own affairs to the end. He attended Mr Jamieson for a week in November 1853, just as ably and satisfactorily as before. We next come to December 1853, by which time he had acquired the habit of indulging in stimulants. The evidence on this point is not perhaps so satisfactory as could be wished, still it does appear that he was in that habit. Dr Steel says so, and in consequence ordered him half a bottle of wine a day to break off the habit. There is no reason therefore, to suppose that Mr Smith of Haughs of Kinnaird gives an exaggerated account of his habits at that time. Dr Christison also said that one result of his inquiries was, that Dr Smith was in the habit of taking more than was good for him. Whatever excitability and irritability may be, there is no proof of insanity up to December 1853. There was no delusion in the grounds of his enmity to Mr Smith, though, as usually happens, these were inadequate. No man is justifiable in such enmity and desire of revenge as he had. There was unquestionably a serious quarrel between him and Mr Smith. Dr Alexander prevented Mrs Smith and Dr Smith from meeting in Edinburgh, in consequence of understanding that it was a condition of the separation that they should not meet. The quarrel between Dr Smith and Mr Smith stood on grounds intelligible, and not inconsistent with what occurs in human life. As to the question, whether he knew he was acting against the law, what is the explanation of his secret arrangement with Campbell, the meeting of him at night, the leaving him at Brechin, the meeting again and again, separating, his going to Brechin and representing himself as robbed—all these circumstances show his consciousness that he was acting against the law. Then laying aside the opinion of Miss Carnegie, that he was not a responsible agent, which is a question for you, all that she says besides is very important to show he knew his guilt in the eye of the law. He confesses to Miss Carnegie, who, he probably expected would assist himself or family, then he says he does not object to tell Officer, but wont tell Smart, as he is a justice of the peace, as that might lead to his apprehension. Is that the view of a man who did not know he was acting against the law? He asks Miss Carnegie what she thought they would do to him He said to Officer: How can they prove it? He had taken his measures as well as any man ever did—and, but for the finding of the piece of stick in the river, it is doubtful if the parties could have been traced out at all. His intention was to go off for the south immediately, and did not expect to find Smart and Officer at Noranside. It is said that it was a delusion of his that he had a sword and pistols. It is clear he had a sword which never has been found at this day. (The reporter remarks, in a note, that the prisoner Smith smiled at this.) This corroborates his statement that he had pistols, and had hid them. The witness Somerville once saw him with pistols. It is said to be a delusion that he was in the bed-room of Mr and Mrs Smith that night. It is not clear that he referred to the night of the fire; still less, if we knew all, that there is not some other explanation of this, than mere delusion. He goes off to Coupar-Angus, in a most rational way. At Forfar the waiter says, there has been a fire at Haughs of Kinnaird. He answers quietly and composedly, "Yes, I heard something of it." He has his luggage directed to his housekeeper's daughter at the station; and all ready, sits late with landlord; he is apprehended before he is up. Is he astonished the officers of the law should come to apprehend? Not at all; he says to procurator-fiscal he knew nothing about the crime. His letter to Miss Carnegie of Sunday night is as correct and well-written as any man could write. No delusions have been proved. The employment of other medical men irritates many practitioners—perhaps it irritated him more than others; and for all the other things said to be delusions, there were some grounds. But though there were, this is not enough. His Lordship then commented at some length on the medical opinions, concluding that the evidence did not come up to what the law required when a plea of insanity was tendered.

The Dean of Faculty said they had been listening for two days to the details of a most melancholy story, the gradual decay of an energetic intellect and a fine moral nature. He entirely agreed with the Lord Advocate that the case involved considerations of the highest possible importance to the country. To detain them with any discussion on the facts of the case, so far as regarded the mere commission of the act charged against the prisoner, would be a waste of time, for they must long ago have seen that his defence was, that when the act was committed, if committed by the prisoner, he was not in a condition of mind to be morally responsible for his actions. He was quite ready to take as his guide the authority to which the Lord Advocate had referred, in so far as it was applicable to the present case, and which he thought might be said to state the law as strongly against him as it could be stated. The English Judges had declared that, to establish a defence on the ground of insanity, it must be clearly proved that, at the time the act was committed, the accused party was under such defective reason from disease of the mind as not to know the nature and quality of the act he was doing, or, if he did know that, that he did not know that he was doing wrong. He (the Dean of Faculty) accepted of that doctrine as applicable to the present case, and he was not in the least degree afraid of having the evidence as a whole tested by this rule. The learned counsel then entered into the history of the case. They had it established that, in 1851, Dr Smith was a man not only in the enjoyment of the most perfect mental and bodily health, but was a man of ability and eminence in his profession, a publicly useful and valuable man, and one who combined with very considerable intellectual powers, and high professional acquirements, a character which stood very high indeed as an amiable and sincere friend. But from being a man of extreme propriety of conduct, esteemed and beloved by everybody, and distinguished by social virtue, he, not suddenly, but gradually, came to alter his habits and mode of living. He became slovenly in his person, violent in his language, immoral in his trains of thought and conversation, and, in short, he presented the very reverse of the picture in 1853 which he did in 1851. All this was to be accounted for by the growing insanity to which the doctors, as early as September 1853, pronounced that his disease tended. This insanity soon became evident to all. In November some of his best friends began to suspect the condition of his mind; at the Haughs of Kinnaird, in December, the Smiths thought he was out of his mind; and in July 1854, Miss Carnegie's observation was—"In short, it appeared to me that the friend I had been accustomed to lean on must now lean on me." The learned counsel, after referring to various scenes and incidents connected with Dr Smith's conduct in Edinburgh in which he gave evidence of being actuated by insane delusions, noticed his statement to Dr Officer about engaging six Highlanders, with dark lanterns, to surround the Smiths' house, and his story to Miss Carnegie about his having gone into their bed-room with three pairs of pistols which would fire twelve shots, that he might accomplish his revenge, but his having relented, thinking it too bad to shoot them in their sleep, a story which he enlarged upon to Dr Brown by saying that he had prepared a train of powder under the bed, and was about to spring the mine when he relented. All these circumstances, the Dean remarked, could only be viewed as the creations of a distempered fancy. He then referred to Smith's conduct in the execution of his purpose, maintaining that his taking with him a drunken confederate in an act in which he needed no associate at all, and that his confession to Miss Carnegie, were not like the acts of a sane man desirous to conceal his crime. Above all, his letter to Mrs Smith was an imprudent act if he wished to escape detection, for it furnished the most conclusive evidence for the Crown in the case. On the whole, he contended that better evidence on a plea of insanity existing at the time of the perpetration of the offence had never been presented to a jury. No doubt, he said, insanity was but a miserable plea to urge. He could not ask them by their verdict to restore Dr Smith to his former position in society. By the visitation of God he had been stricken with a malady which was the most grievous of all; but though they could not restore him to his former state, they had nevertheless a most important duty to perform. They could do justice in this case. They could affirm, and he apprehended they were bound to do it, that when he committed this offence he was absolutely bereft of reason.

Mr Patton was about to address the jury in behalf of Campbell, but the Lor Justice-Clerk said he did not think that it was necessary.

The Lord Justice-Clerk in his charge said, that while it was right for those who administered the law to attend to the principles of jurisprudence by which the question of punishment was to be regulated, or the liability to punishment determined, these were not considerations for the jury. Their duty was to say upon the facts, whether the insanity of the prisoner was established; they had to apply the law as laid down by the Court on that part of the case. They must not be too much affected by the gradual declension of a person of talent and respectability into the state in which he was found at last, if they were not satisfied that that was the result of insanity. They must distinctly remember that it lay with the prisoner to prove fully and satisfactorily that he was not liable to punishment, in respect that when he committed the crime he was bereft of reason. If that defence were not found tenable, the Crown was entitled to their verdict. Common-sense justified the rule of law, which required the prisoner to prove this plea, and they were only acting properly and fairly in requiring that it shall be clearly made out. He (the Lord Justice-Clerk) did not intend to enter into the law of the case so fully as he had done in that of Gibson of 1844, as referred to by the Lord Advocate. To the charge he then gave he adhered to in all respects; and he was glad that that charge had been taken by the best English writers on medical jurisprudence as an exposition of the law on the subject. In the first place they must understand that the law did not for one instant countenance the notion of moral insanity — that was to say, what was called irresistible impulse, by which a man was driven into crime, while it was not proved that his reason was destroyed. That perversion of moral feeling was not insanity. The view of such cases taken by the law was the doctrine of the Bible, that if a man gave way to temptations, which were strong only because he had long indulged in evil thoughts and angry passions, he was not tempted above what he was able to bear; and, unless there was an absolute aberration of reason, the law held that he could resist, and must resist, promptings to commit an act contrary to law. Then, again, there was no such thing admitted in law as partial insanity, call it monomania or anything else. As was well said by Lord Brougham, if the mind were unsound in one point it was unsound in every respect so long as that which caused the unsoundness existed in the mind. It was not necessary, however, that insanity should be continually and constantly manifested, for a man might be insane at particular times—at one time a fit object of punishment, and at another an unfit object of punishment. The ordinary instances of this were to be found in what were termed "lucid intervals." But the jury must understand that absolute alienation of reason must be proved, a principle well explained by Baron Hume, and not less important in the present case, because that profound thinker and excellent lawyer connected it with delusions. He had to remind them that the jury in such a case as this were far better judges of what insanity was than either medical men or lawyers. While he had told them that the pannel must be bereft of reason to be exempted from punishment, they were not to suppose that that implied a state of demoniacal fury. Another consideration was, whether the insanity was heard of for the first time after the commission of the crime, or whether weakness of intellect had been going on progressively, with physical disease, for a length of time, and resulted gradually in alienation of reason. With regard to the prisoner Campbell, if they should be satisfied that the act was committed by Dr Smith, under the influence of insanity, he (the Lord Justice-Clerk) did not think they would be inclined to bring in Campbell guilty of the act without Dr Smith. The evidence against Campbell was as bare as it could well be, and it would have been infinitely better had he been produced as a witness, as he might have told what Dr Smith did on that night. His Lordship then proceeded to review the evidence, and his comments upon the leading facts of the case were favourable to the special defence set up for Dr Smith. His address lasted, on the whole, above four hours.

The jury retired at a quarter past five o'clock, and, after ten minutes' absence, returned the following verdict:—

"The jury unanimously findthat the pannel George Lillie Smith committed the act of fire-raising mentioned in the libel, but that he was insane at the time of doing so; and find the pannel Robert Campbell not guilty."

INDEX.

The large Figures refer to the pages of the Case-Book.

Amputation of the foot, case of, by John Traill, Esq., . . **21**
Aneurism, abdominal, two cases, by Dr W. T. Gairdner, . **12**
Anæemic palpitation, . . **33**
Aneurism, cases of, by Professor Miller, ... 33
Apoplexy during labour, case of, by Dr W. Williamson, . **11**

Begbie, Dr, case of anæmic palpitation with enlargement of the thyroid glands, and eye-balls, **33**
—— account of cases treated in the Cholera Hospital, Edinburgh, ... 250
Begbie, Dr J. Warburton, on facts and opinions in regard to Asiatic cholera, ... 86
Bladder, presentation of, in labour, **30**
Blow-pipe, the use of, reviewed, 339
Brodie, Dr, case of lupus, . . **31**
Butcher on wounds of arteries, 166
Burmah, on the climate of, &c., by Dr C. Murchison, . . 62, 205

Cancerous disease successfully treated by escharotics, by Dr James D. Gillespie, . . **8**
Chiari on anomalous pregnancies, 148
Chisholm, Dr John, on veratria in tic, ... **2**
—— on anomalous acute disease of skin, ... **4**
Cholera, the, in Berwick Street, by the Senior Curate of St Luke's, reviewed, ... 137
Cholera, facts and opinions in regard to, by Dr J. Warburton Begbie, ... 86
Cholera, account of cases treated in the Cholera Hospital, Edinburgh, by Dr J. Warburton Begbie, ... 250
Croup, case of, following measles, by Dr C. Weber, . . **23**

Dalrymple, John, Pathology of the Eye, reviewed, . . . 323
Diseases of Infancy and Childhood, by Dr C. West, reviewed, 108
Drunkenness, Pathology of, by Dr C. Wilson, reviewed, . . 329
Duncan, Dr J. Matthews, on the statics of pregnancy, . . 45
—— case of labour complicated with hydrocephalus of child, . **20**

Elbow-joint, cases of, . . 154
Eye, Diseases of, by Dr Mackenzie, reviewed, ... 317
—— Pathology of, by John Dalrymple, reviewed, . . 323
—— Iconography of, by Sichel, reviewed, ... 326
Eye-balls, enlargement of, . **33**

Forbes, Francis, trial of, accused of the murder of Ann Harvey, 160
Fore-arm, comminuted fracture of both bones of, ... 158

Gairdner, Dr W. T., two cases of abdominal aneurism . . **12**
Gangrene, spontaneous case of, in a child of 8 months, by Mr Sidey, ... **7**
Geology, Page's Introductory Text Book to, reviewed, . . 136
Gillespie, Dr J. D., case of cancerous disease successfully treated by escharotics, . . . **8**
Glandular enlargement, with exanthematous eruption, . . **37**
Goitre, congenital, case of, . **32**

Harvey, Dr A., presentation of bladder in labour, . . **30**
Hydrophobia and Rabies, Dr Kemp on, ... 1

Index finger, excision of, . . 158
Iodine, antiseptic properties of, 160
—— injection of, in snake-poison, 159

Joint, condition of after resection, 154

Keiller, Dr Alex., on spurious pregnancy and hysteria, . . 19

Keiller, Dr Alex., case of congenital goitre, ... 32
Keith Dr Alex., case of induction of premature labour, . . 39
Keith, on excision of knee-joint, 155
Kemp, Dr T. Lindley, on Rabies and Hydrophobia, . . 1
Knee-joint, second case of excision of, successfully treated, by Dr W. Keith, . . . 155

Lactation, undue, on retinitis from, 146
Labia pudendi, sloughing of, . 25
Lupus of face, fatal by laryngeal disease, ... 31

Mackenzie, Dr Richard, brief memoir of, ... 281
—— on retenitis from undue lactation, ... 146
—— *Dr Wm.*, on Diseases of Eye, reviewed, ... 317
Materia Medica and Therapeutics, by Pereira, reviewed, . . 132
Miller, Professor, cases of aneurism, 33
Murchison, Dr Charles, on the climate, &c., of Burmah, . 62, 205
Muspratt, Dr S., on blow-pipe, reviewed, ... 339

Neuralgia, on a new method of treatment of, by Dr Alex. Wood, 265

Page, David, Esq., his Text-Book of Geology reviewed, . . 136
Pereira's Materia Medica reviewed, 132
Portio dura, paralysis of, . 159
Pregnancies, anomalous uterine, 148
Pregnancy, on the statics of, by Dr J. Matthews Duncan, . 45
Pregnancy, spurious and hysteria, Dr A. Keiller on, . . 19
Profunda femoris, wound of, . 156
Psychological Inquiries, reviewed, 115

Retinitis, on, from undue lactation, by Dr W. Mackenzie, . 146

Seyfert, Dr Bernard, prolapsus of uterus, cured by retroflexion, 151
Sichel, Iconography of Eye, reviewed, ... 326
Sidey, Charles, Esq., case of spontaneous gangrene in a child eight months old, . . . 7
Sigmund, Professor C., Report of syphilitic cases treated at the large hospital of Vienna in 1852 and 1853, ... 151
Smith, Dr Archibald, the rise and progress of yellow fever in Peru, ... 165
Smith, Dr G. Lillie, trial of, for wilful fire-raising, . . 341
Society, Medico-Chirurgical of Edinburgh, proceedings of session 1853–54, . . . 141
Sutherland, Mr Geo. S., case of exanthematous disease, . . 37
Syphilitic cases, report of those treated at the large hospital of Vienna, in 1852 and 1853, . 151

Throat, extensive suicidal wound of, recovery from, . . . 158
Thyroid gland, enlargement of, 33
Tibial artery posterior, wound of, successfully treated, . . 157

Ulnar artery, wound of, successfully treated, ... 157

Vagina, sloughing of, followed by stricture, ... 25

Weber, Dr C., case of croup following measles, ... 23
West, Dr Charles, Lectures on Diseases of Infancy and Childhood, reviewed, ... 108
Williamson, Dr. W., case of apoplexy during labour, . . 11
—— case of vaginal stricture, . 25
Wilson, Dr Charles, Pathology of Drunkenness, reviewed, . 329
Wood, Dr Alex., new method of treating neuralgia, . . 265

END OF VOLUME EIGHTY-TWO.

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CASE-BOOK

OF THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

[The CASE-BOOK of the EDINBURGH MEDICAL AND SURGICAL JOURNAL is intended to receive such communications in practical Medicine, Surgery, and Midwifery, as appear to be of general interest, *from the importance of the facts observed*, apart from the views which may be entertained or expressed with regard to these facts. It is not intended, by this definition of its object, to restrain the expression of opinion on the part of those who may be kind enough to furnish us with valuable observations; on the contrary, commentaries arising directly out of the facts, and written in a scientific spirit, are expressly invited, as well as the bare materials of thought. But it is believed by the conductors of this Journal that many really important observations of a practical kind escape being recorded (especially in country practice), to the great detriment of science, from the want of a medium in which they may be advantageously stored up, without being either woven into elaborate memoirs, or lost among the trivial and diffuse productions that form so large a proportion of periodical literature. To all records of medical observation in which facts are stated, either novel in themselves, or tending to important conclusions on points not already sufficiently canvassed, the pages of the Case-Book will be open, provided, of course, the multitude and length of the details be not out of proportion to their inherent importance.

The Case-Book of the MEDICAL AND SURGICAL JOURNAL will be paged separately from the work of which it forms a part. Its pages will be numbered in a different type, which can be clearly recognized in the annual index to the volume. It is intended that one or more sheets shall be given with each number, which may be collected and bound either at the beginning or at the end of the volume. With the last sheet will follow an accurately-compiled synoptical index, or table of contents, on a new plan, which will greatly facilitate reference to the details of observation communicated in the Case-Book.]

I.—*Cases Illustrating the Effects of the Local Application of Veratria in Tic.* By John Chisholm, M.D., Ludgate-Lodge, Ratho.

The sudden spontaneous improvements which neuralgic affections undergo in some instances, their obstinate resistance to all remedial measures in other cases, and the very opposite methods of treatment by which they occasionally seem to be benefited, have led many practitioners to look with distrust upon the employment of various remedies, which are habitually prescribed by others with considerable confidence. Among these, we believe that the local application of veratria in the treatment of Tic, and of some other painful affections, has been too much neglected; for while its external use affords relief in a considerable proportion of cases, it does not in any way interfere with the internal administration of other remedies.

In order to give this drug a fair chance of success, it is necessary that its physiological action be fully induced in the affected part. This is announced by a peculiar pricking sensation analogous to, though not identical with, the tingling produced by aconite; it usually commences after the ointment has been rubbed in for ten or twenty minutes, and generally continues for three or four hours.

Many failures are probably to be attributed to the small quantity of ointment used; over a part one inch in diameter, a drachm at least ought to be applied. A very necessary precaution is to procure a genuine unguentum veratriæ, for we have found that the druggist sometimes supplies it perfectly inert.

In cases in which the attacks recurred at regular intervals, we have generally desired the patient to have the ointment applied half an hour before the expected paroxysm, and renewed whenever its physiological effects begin to wear off; in other cases where the paroxysms were frequent or irregular, we have endeavoured to keep the part constantly under the influence of the drug. The strength of the ointment used was sometimes fifteen, and sometimes twenty grains to the ounce.

We have tried the veratria in six cases of Tic. The details of three are given below. Of the others, one, a delicate young woman, with irregular menstruation, in whom the attacks were frequent and severe, used it for some time without benefit; she ultimately got well under the use of the saccharine carbonate of iron. Another, a ploughman, had a paroxysm every twelve hours; the employment of the veratria ointment always gave him relief, but as the attacks continued to recur regularly at the end of three days, we ordered him large doses of quinine, which speedily removed the disease. The third, a middle-aged woman, had for several years been a dreadful sufferer from Tic in the lower jaw, and infraorbital space on the left side; she had occasionally experienced intermissions of the disease for a month or two at a time, but it had always returned with unmitigated severity. When I prescribed the veratria for her she had been suffering from paroxysms for upwards of four months; a fortnight's use of the ointment left her quite well, and she now writes that during the last two years she has been subject to returns of the pain, which however have always yielded to the application of the veratria.

Case 1.—Mrs D., æt. thirty-eight.—I first saw Mrs D. in December 1852. She had been in very delicate health for some years, the result of several abortions, after which she lost much blood; for the last nine months she has suffered from violent Tic, principally along the lower jaw of the left side, and sometimes in the left temple. The paroxysms are very frequent; sometimes they pass away in a few seconds; at other times they continue for hours: during their continuance there is generally a copious flow of saliva from the mouth on the affected side. The attacks often come on without any apparent exciting cause; but the slightest current of air, loud noises, and the acts of speaking or eating, are sure to induce a paroxysm. She is now entirely confined to bed from debility, which she attributes to the impossibility of taking food in sufficient quantity; her only other complaint is obstinate constipation of the bowels, which are never moved without the use of laxatives. For the removal of her disease, purgatives, quinine, iron, and other internal remedies, have been tried without any marked benefit; and for the last month she has been using no remedy at all. I advised her to try the veratria, desiring her to use it three times a-day; this she did, and soon experienced such benefit that she commenced to apply the ointment so often as to keep the part constantly anæsthetised. After three weeks of this treatment she discontinued the ointment, and has continued well ever since. It is right to mention, that she commenced a course of the saccharine carbonate of iron immediately after discontinuing the veratria.

Case 1.
Previous Health.
Symptoms of Tic.
Salivation.
Exciting Causes.
Inanition and Constipation.
Treatment.
Veratria.

Case 2.—On the 6th January 1853, I was called to see John A., a groom, æt. twenty-four. After a fit of dissipation at the New-Year he had been seized with Tic over the right orbit. The attacks came on regularly at 9 A.M., and continue till 4 or 5 in the afternoon. The exacerbations are so severe as to make him cry out. His tongue is loaded with a creamy white fur; his bowels confined, and his appetite quite gone. The other functions are natural; ordered him two aloetic pills, each containing one drop of croton oil.

Case 2.
History and Symptoms.
Treatment.

Veratria.

January 7.—Saw him about 5 P.M., and found that, although he had been purged very freely yesterday, the pain had returned equally severe at the usual hour this morning, and continued till about one hour ago. To commence the use of the veratria ointment to-morrow morning at 8½ A.M., and to renew it frequently during the day.

January 8.—Saw him about 2 P.M., and found him comparatively comfortable,—the veratria had produced the usual pricking sensation; but suddenly about 9 A.M. he felt as if a great weight had started into the part where he used to have Tic. This feeling still continued, but gradually disappeared about one hour after my visit.

January 9.—Continues the use of the ointment. The same feeling of a weight over the orbit is present to-day—it came on about 10 A.M., and is less disagreeable—he is now able to take his food well, and has no complaint. He continued to use the ointment for some time after this, and was soon able to resume his work.

Case 3.
History and Symptoms.

Case 3,—Alexander N,, æt. thirty-two, a quarryman.—After exposure to cold on the 15th of April 1853, he was seized with a severe pain over his right orbit. When I visited him on the 18th, found him suffering from Tic; it had recurred regularly at 2 P.M., and continued a few hours. His appetite was good, and all the functions natural—ordered him to apply the veratria ointment; he felt the pain a little for the first and second days, but was quite well afterwards, and resumed his work within a week.

Veratria.

II.—*Case of Anomalous Acute Disease of the Skin, resulting in exfoliation of the Cuticle.* By JOHN CHISHOLM, M.D., Ratho.

Previous Health, &c.

On the 10th of July last, I was called to see James ——, aged 53, a labourer, he is a strong muscular man and hitherto has generally enjoyed good health.

History—Febricula.

Two days ago he was engaged working in a ditch, when he got his feet wet and felt very cold during the whole afternoon; ever since he has been dull and listless, complaining of aching pains in his back and limbs, with loss of appetite and inaptitude for exertion; he has not been confined to bed.

To-day, he complains of headache and a "soreness" over his whole body, his pulse is 80, the tongue is moist and covered with a brownish white fur, all the other functions are natural.

To have Pulv. Doveri gr. x., and hydg. c. creta gr. v. at bed-time, and an aperient draught in the morning.

Diarrhœa.

Visited him again on the 12th, when I found that he had ventured out to take a walk on the 11th, and was seized with a violent rigor; he soon became very feverish, and during the night had five or six loose stools, with griping in the bowels; in the morning he discovered that his body was all red.

Red exanthem on trunk of body,

Now, the whole trunk of the body is of a bright scarlet colour, it is quite uniform throughout its whole extent, but of a deeper hue over the back and parts subjected to pressure. There are no elevations or paler portions, it does not pit on pressure, but when the finger is removed it leaves a pale spot which instantly becomes red again. Everywhere around the margins the redness is gradually lost in the surrounding healthy skin, except along the under border of the lower jaw where it ceases abruptly; even here the affected portion is not perceptibly raised above the normal skin. This redness covers the front of the neck, the back, thorax, and abdomen, gradually dying away over the origin of the deltoids, and at the groins and crista ilii. The parts are slightly tender on pressure, and he complains of an intense burning heat in them.

Mouth and tongue.

The tongue and interior of the mouth are bright red, the redness extends as far back as the velum palati, the tonsils are unaffected; scattered over the interior of the mouth are a number of vesicles about one line in diameter, many of which have a shrivelled appearance as if only half full of liquid; they are most numerous along the edges of the tongue; the papillæ of the tongue are not more distinct than usual.

Fever.

He is now highly feverish, complaining of intense headache and urgent thirst, with total loss of appetite. His pulse is 120, full and strong, he has a frequent desire to micturate, the urine is scanty, depositing an abundant sediment of a brick-red colour, he has no local pain. He is to take a tablespoonful of the following mixture every two hours.—℞. Vin. Antimon. ℥ss., Spt. Aeth. nit. Tinct. Opii. Camph. ā. ā. ℥i. Aq. fontis, ad. ℥viii., misce.

Bladder—Urine.

Uric acid in crystals.

July 13.—He perspired very freely during the night, principally over the face and limbs, but on the body also, he says. The fever has very much abated, the headache is nearly gone. The pulse is 96 and much softer, the thirst is not mitigated, and the irritability of the urinary bladder has increased, so that he requires to pass urine every few minutes, and very small quantities at a time; it is not albuminous, is highly acid, and now deposits a red sand, which is found to consist of crystals of uric acid; it contains not more than the usual quantity of cast-off epithelium.

Progress of eruption—Vesicles.

The redness of the skin is less bright since yesterday; it has not increased in extent, but a number of vesicles are studded over the affected part; the largest are scarcely one line in diameter, most of them are about one-half that size, and some are mere dots. They are of an opaque milky-white colour, and when punctured, a turbid milky-white fluid escapes, which somewhat resembles lymph mixed with pus, and coagulates spontaneously. The vesicles are largest and most numerous at the flexures of the body, on the breast they are generally connected with the roots of the hairs. All the vesicles in the mouth have disappeared, leaving portions denuded of the epithelium, and the remainder of the epithelium seems separating rapidly.

He is to take a dose of his mixture every four or five hours, and to have linseed tea and other demulcent drinks *ad libitum.*

Decline of fever, &c.

July 14.—The fever has almost entirely left him, his pulse is 80, the thirst is less urgent, but he has still no desire for food, the seve-

Confluence of vesicles.

Eruption spreading to extremities.

rity of the urinary irritation is rapidly subsiding. The redness on the trunk is declining, the vesicles are not increased in number or size, but in several places the cuticle has a wrinkled appearance, and is very loosely connected with the true skin, for it can be moved upon it. The fluid in the vesicles, also, at various parts, can be forced along under the cuticle for a considerable distance, so as to unite several into one. To-day there is an accession to the redness, it has spread over the arms and thighs. On both arms it reaches a little beyond the elbows, and seems to have spread from the body by contiguity; on the right thigh also, where it reaches scarcely so far as the knee, it is connected with the affected skin on the trunk; but on the left thigh, a portion of sound skin intervenes between that of the trunk and that recently affected. Here the redness reaches below the knee, and is spreading upwards towards the body; on the internal aspect of the thigh it reaches within an inch or two of the pubes; externally it ceases about midway between the knee and trochanter; the redness here has no distinct border, and is very irregularly blended with the unaffected skin. The redness upon the limbs in all respects resembles that upon the body, when it was at the same stage, except that a few clear vesicles are seen shining through the cuticle.

General symptoms—Urine, &c.

Separation of cuticle.

July 15.—The restlessness and want of appetite still continue; he is now passing urine copiously, and it deposits no sediment; his pulse is natural, and he experiences no pain except from the tenderness of the skin. On the trunk, the cuticle is separating in large sheets; to-day I stripped off the whole cuticle of the abdominal parietes in one unbroken membrane, this occasioned little or no pain; there is a little moist exudation underneath the cuticle on the back part of the shoulders, and over the sacrum, where there is a good deal of irritation; a considerable exudation of serum has taken place—the linens are quite stiff with it. On the left thigh, the redness has spread *upwards* as far as the trunk. On the limbs generally the affection does not seem so intense as it was upon the body; the vesicles are smaller and fewer, being confined to the more delicate portions of the skin. To have the back covered with raw cotton.

Emaciation—Anorexia.

Progress of exfoliation, and diffusion of eruption.

July 18*th*.—Since last report he has been losing flesh rapidly, and though he is free from fever, and all the functions nearly natural, he takes almost no food; the integument of the trunk is, for the most part, nearly well now. On the breast, the cuticle is separating a second time, but now in large scales. On the limbs, those portions first affected are losing the cuticle, very much in the same way as it separated from the body. The redness is spreading slowly and insidiously downwards towards the hands and feet, but is not nearly so bright as it was upon the body, and no vesicles are visible below the knees.

Face and conjunctivæ.

July 19*th*.—Was threatened with a rigor yesterday; he is somewhat feverish to-day, and complains of headache. The integument of his face and ears is bright scarlet, and small clear vesicles are seen shining through the cuticle. The conjunctivæ also are effected, in appearance closely simulating ordinary conjunctivitis; the integument on the extremities is scarcely changed since yesterday.

July 24*th.*—The cuticle of the face had entirely separated two days ago, the conjunctivitis subsided about the same time; on the limbs the cuticle has entirely separated as far as the ankles and wrists; where the edges of the old cuticle present a ragged uneven appearance, small portions of it can be detached, but it is firmly adherent. **Exfoliation.**

About three weeks after this, and after he had begun to walk about, the whole cuticle of his feet separated entire, the cast-off skin of each forming a complete slipper; the nails did not fall off; nearly at the same time the cuticle of the palms of his hands separated. At this time he was much emaciated, and gained flesh and strength very slowly, but he ultimately got quite well. **Feet and hands.**

Mr Craig informs me, that in November 1850 he had an attack of a similar nature. At that time it came on after the exhibition of croton oil for very obstinate constipation; its course was much more rapid, and there was a large quantity of fluid exudation after the separation of the cuticle. **Previous attack of similar affection.**

III.—*Spontaneous Gangrene in a Child Eight Months Old.*

By Charles Sidey, Esq., Surgeon, Edinburgh.

The child was passing through a second attack of hooping-cough, and the disease presented no unfavourable symptom till, on the 22d of May 1854, there was observed considerable feverishness, with bilious diarrhœa. The breathing was unaffected. In the evening of the 23d a reddish ring appeared round the left thumb. A few hours afterwards, this part become quite black, and this dark colour soon spread over the whole hand. The left half of the scalp and the corresponding ear at the same time assumed a purplish dark colour, which during the night spread over the entire scalp. In the morning, on examination, the scalp, both cheeks, and both hands, were gangrenous; notwithstanding which extensive disease, the child remained sensible, and continued to suck until twelve hours after the first appearance of the disease, when it expired. **Hooping-cough.** **Febrile Symptoms.** **Gangrene.**

[This case is probably unique, as regards the localities and diffuse character of the gangrene. It differs from all the recorded cases of infantile gangrene, noma, cancrum oris, or of the German *wasser-krebs*, to which we have had access. The reader may consult the treatise of MM. Rilliet and Barthez (3d edition), which contains an extremely full notice of the authorities on the gangrene of infants. We are not aware of any case of gangrene of the scalp, except from injury, either in infants or adults—Editor.]

IV.—*Case of Cancerous Disease, successfully treated by Escharotics.* By JAMES D. GILLESPIE, M.D., F.R.C.S., Edinburgh, Assistant-Surgeon to the Royal Infirmary.

Mr G., the subject of the following case, is 79 years of age. He is a man of large powerful frame, of regular habits, and till within the last few years has always enjoyed robust health.

History—Abrasion and ulcer of heel.

About six years and a half ago (in 1847), Mr G. met with a slight abrasion of the heel, which soon cicatrized. The cicatrix, however, some time afterwards gave way, causing a small ulcer on the posterior prominence of the os calcis.

Warty growth.

It continued occasionally healing up and breaking out again for some months, till at last a hard warty substance grew on it, which he was in the habit of getting pared.

In 1849, a hard flat tumour occupied the situation of the cicatrix. He was then living at Portobello under Dr Hill's care.

In May 1851, the tumour presented all the characters of a hard cancerous growth, was about an inch in diameter, round, raised about half an inch from the surface of the heel, and painful when touched.

Erythema. Increase of tumour.

Professor Syme was called into consultation, and it was determined that an attempt should be made to eradicate the tumour with caustic. Before any application, however, was made, an erythematous inflammation of the leg took place, extending up the thigh to the groin, causing swelling and tenderness of the glands. The tumour also increased considerably in size.

Second attack of erythema, with increase of tumour.

When he recovered from this attack he went to Perthshire, and while there had another severe erythematous affection of the leg and thigh, in consequence of imprudent exposure.

Professor Syme and Dr Gillespie visited him while labouring under this attack, and found him in a most excited state. The tumour was much increased in size, and presented an irritable surface; the glands in the groin were swollen and tender, and erythematous stripes extended up the limb from the tumor to the groin, along the course of the lymphatics. The general health was much impaired, and the mental faculties were greatly affected. He was very nervous, unable to articulate distinctly, and frequently miscalled the words he tried to make use of.

Application of caustic (chloride of Zinc.)

He returned to town in September, and on the 4th of October 1851 the first application of caustic was made. The tumour was then somewhat larger than a full-sized walnut, round, of a spongy character, and occasionally bleeding profusely. A paste, composed of the chloride of zinc and flour, was applied, and kept on for 18 hours. Some days thereafter the prominent portion of the tumour came away in an entire mass, an inch in thickness, and the granulating surface appeared to take on a healthy action; but soon after cicatrization had commenced the morbid growth reappeared on a larger base. For the space of a year the same thing occurred, powerful escharotics being applied to destroy the tumour, but always with the same result, its reappearance on a larger base. Intense pain was very frequently felt

Caustic repeatedly applied. Recurrence of tumour.

Glandular enlargements.

during the intervals of the application, and bleeding from the spongy fungus repeatedly recurred. The glands in the groin became permanently hard and much enlarged, but fortunately not painful. Often nature made vigorous efforts to throw off the disease, the granulating sore for a long time continuing healthy and cicatrizing kindly.

Chloride of zinc and potassa fusa compared.

Chloride of zinc paste was applied seventeen times, potassa fusa energetically rubbed in thrice. Professor Syme, I may mention, was present on all these occasions. The chloride of zinc proved much more efficacious, though slower in its action, than the caustic alkali; for though the latter was rubbed in strongly, and made to pierce the tumour in every direction, it never seemed to bring anything away, the portions which must have been destroyed being rapidly filled up. The chloride of zinc was employed undiluted with only sufficient flour to give it consistence.

After the application of September 18th 1852, the tendency to reproduction appeared to be diminished, though about a fortnight before potassa fusa had been freely applied, with no good effect; indeed it was noticed that after the application of this caustic a more excited reproductive action was established.

Last application of escharotics.

Recovery.

Soon after the slough had come away, caused by the application of September 18th, and before the tumour had risen to any degree, a fresh attack was made on it, and on October 4th 1852, exactly a year, to a day, from the time when caustic was first employed, the 20th and last escharotic application took place. The slough which came away on that occasion left a circular excavated sore, having a diameter of fully three inches and a half. A portion of the os calcis which was laid bare exfoliated. From that time the morbid growth ceased to exist, the wound granulated healthily though slowly, gradually contracting in size, the hard abrupt margins becoming rounded, and a healthy cicatrix growing steadily from the entire circumference. In the course of some months the wound was entirely healed, and from that time to the present, a period of two years, there has been no tendency to reproduction; the glands in the groin, though still enlarged, have become reduced in size, and give no uneasiness; and the foot, with the exception of having a depression on the posterior and inner aspect of the heel, is as natural as its fellow.

I have brought this case under the notice of the Society, as I consider it to possess some points of interest, bearing more especially on the treatment of a class of tumours in which surgeons, when they cannot have recourse to operative interference, are inclined to adopt merely palliative remedies. That the tumour of Mr G. was essentially a malignant growth, no one who has heard the details will deny; and, that it was not a case for amputation, the age of the patient, his nervous temperament, and the enlarged glands in the groin, sufficiently show. Had the tumour been left to itself, or merely palliated, there can be no doubt a fatal termination would soon have ensued.

As a last resource, caustic was applied, and steadily persevered with; latterly, indeed, with very little hopes of an ultimate cure, and more in order to allay occasional excruciating lancinating pains, and to arrest the hæmorrhage, which was becoming alarmingly frequent.

The sudden amendment which took place, when we had begun to despair of a favourable result, is, I think suggestive of serious consideration.

At a time when empiricism is endeavouring to claim as its own the treatment of cancer, and almost every newspaper teems with the most incredible cures being obtained by the vaunted agency of some secret infallible specific, I think it behoves the surgeon of the present day to look more narrowly into the effects of external remedies, ere he pronounces the fiat which, because he cannot make use of the knife, dooms his patient to a lingering and agonizing death. I do not for a moment wish it to be thought, that I believe all the wondrous statements of the empirics who prey on public credulity; but, it is just possible that a case may occasionally fall into their hands, where the surgeon has wisely declined to operate, and, perhaps unwisely, neglected to make trial of escharotic treatment.

It is still farther from my intention to propose that caustic, and more especially the chloride of zinc, should supersede the removal by operation of the diseased parts, as I firmly believe, in the vast majority of cases, the latter to be the proper mode of procedure; but there may be special cases, like the one I have had the honour of reading to the society, where energetic application of caustic is worthy of a legitimate trial.

It may be that the disease may return in the case previously detailed, though the glands in the groin have much diminished in size, and lost their almost stony hardness; but at the present moment, above two years have elapsed since the tumour was destroyed, without the smallest indication of a recurrence of the malady, and, it is no small matter to ascribe to the art of surgery, that a life of usefulness has been preserved already for three years from apparently inevitable and impending destruction by a disease so deadly and so agonizing in its character.

[This case was read before the Medico-Chirurgical Society of Edinburgh, December 6th, 1854.]

V.—*Case of Apoplexy during Labour, fatal without convulsions. Extensive extravasation into the Brain.* By W. WILLIAMSON, M.D., Physician to the Royal Infirmary, Aberdeen, &c.

I received a hasty summons one morning, about twenty minutes before four, to give my assistance in the case of a poor woman, who had had lingering labour pains during the two previous days.

On arriving at her bedside at five minutes past four, I found that she had expired about ten minutes previously.

History of the case.

A vaginal examination detected the child's head presenting, and the os uteri well dilated.

From the midwife, who had been with her during her labour, and who is a very intelligent person, I gathered the following particulars:

Headache.

She had been in attendance on deceased for two days previous to death, and though the labour was lingering, nothing was observed to cause the least anxiety as to the result of the case. The deceased had complained of a constant pain in her head for at least three weeks previously, but it was never so severe as to attract particular notice. At half-past two A.M. the liquor amnii was discharged.

Symptoms.

An hour later (3.30) the character of the breathing was observed to have undergone a change, and to be more of the nature of sighing.

The midwife went to the bed, and examined her patient's countenance. Her features were calm and composed. She seemed to be in a deep sleep, and as she spoke to her only five minutes before, when she repeated correctly part of a psalm, she did not disturb her, thinking it impossible that she could be in any danger.

About ten minutes afterwards (3.40) all at once she ceased to breathe. The midwife became alarmed, and despatched a messenger for me; but it was too late.

There was no convulsion, no expression of sudden acute pain in the head. Only she had complained very much of the motion of the child about four the previous afternoon.

An account somewhat different was given me by a woman who had been in the room with the deceased during her labour, and is to the following effect:—About one o'clock of the afternoon before her death, she partook of a light dinner, and almost immediately after she became sick and vomited violently.

In a short time she complained of a severe pain in her head; vomiting recurred frequently during the day, and was of a different kind from what she had ever seen in labour, being of a much more violent and distressing nature. In the evening the poor woman began to entertain great fears of herself, said she would die, and was anxious to see her friends.

About four P.M. she complained very much of the motion of the child, but not after that. The character of the breathing became altered about twelve at midnight, and bore a strong resemblance to sighing. She did not speak sensibly after half-past one A.M., "with the exception of repeating some verses of a psalm."

The respiration ceased suddenly. There was no convulsion. The labour pains were not of unusual severity.

Her sister informed me that deceased had been very intemperate for the last eight years, and that she had lived on very bad terms with her husband for some time.

Sectio cadaveris. *Sectio Cadaveris* 26 hours after death. Present, Drs Ogston, Reid, and Redfern.

The body was full and well-developed, surface pale, except the lips, neck, and dependant parts of the trunk, which were livid; slight œdema of the feet. Fingers half bent, thumbs extended. Blood was found in considerable quantity beneath the scalp, and about four ounces of the same fluid flowed from the right hemisphere of the brain, from an accidental cut with the saw. On removing the upper portions of both hemispheres, the brain presented a healthy appearance, of perhaps a paler hue than natural, but nothing unusual was remarked until the lateral ventricles were laid open, when they were both found to be filled with blood, partly fluid but chiefly coagulated.

The left thalamus and corpus striatum could scarcely be recognised, and in their place existed a soft reddish grumous mass. The surface of the right thalamus and corpus striatum was congested.

The extravasation extended from one lateral ventricle to the other, the septum lucidum was broken down ; as was likewise the floor of the third ventricle, through which the blood found its way to the base of brain. Several minute clots of blood were found in the substance of the pons varolii.

The cerebral arteries were not remarked to be diseased. The heart pale and flaccid, ventricular and aortic orifices healthy.

The womb contained a full-grown male fœtus, which it was found impossible to extract by making traction on the neck, but which was withdrawn with great ease by seizing hold of the lower extremity.

There are three points worthy of remark in this case :—

1. **The absence of convulsion.**
2. **The short period which elapsed between the patient's speaking and her death.**
3. **The absence of coma.**

In regard to the second of these, it is true that there is a discrepancy between the statements of the midwife and the woman, but they both agree on the main point "that the deceased did repeat correctly and sensibly a portion of a psalm about twenty minutes before death."

VI.—*Two Cases of Abdominal Aneurism.* By W. T. Gairdner, M.D., Physician to the Royal Infirmary of Edinburgh.

Case 1. Case 1.—A groom, aged thirty-one, came under my care in the spring of the present year (1854), under the following circumstances :—

He was considerably emaciated, though not pale; the muscles and fat were very much diminished, the abdomen sunk, the features pinched and anxious. He had been for a length of time almost bedridden in consequence of extremely severe suffering referred to the upper part of the loins, during the paroxysms of which the recumbent posture was alone tolerable. The pain he described as centering about the region of the kidneys, especially on the left side, and stinging downwards into the left testicle, which, he said, was often retracted. The whole abdominal walls were frequently involved in the painful sensations; the extremities were comparatively free, but motion of the lower limbs invariably aggravated his sufferings, while perfect rest somewhat relieved them. His appetite was very bad, the breath sour and slightly fetid, the tongue frequently loaded but sometimes clean, the gums spongy, and presenting to a marked degree a red or violet-coloured congestion of their margins; the bowels were frequently costive, but not on the whole very irregular considering the small quantity of nourishment he took; the evacuations were dark-coloured. His pulse, though variable in frequency, was small and weak, and neither pulse nor skin indicated any appreciable fever. The urine was repeatedly examined; a few crystals of free uric acid were once or twice discovered; in other respects it was normal.

Symptoms.
Emaciation.
Lumbar pain simulating "nephritis."
Its characters.
Appetite, &c.
Tongue, &c.
Alveolar margin.
Bowels, &c.
Pulse, &c.
Urine.

The history of his complaints dated from 1847, when he had typhus fever, having been up to that period a healthy man. While convalescent, he experienced the first attack of his pain, which did not however last long. He believed that his urine at this time contained blood. After recovering from the fever, he continued on the whole well; but the pain in the loins occasionally recurred. In 1849 he had a fall from a horse, which slightly strained him, but produced no permanent inconvenience.

History.
Typhus Fever.
First attack of pain.
Injury.

In September 1853, a severe attack of pain, which at this time was felt in the left side and in the testicle, drove him from his occupation. He took refuge, on two occasions, in a county infirmary, where he was treated by leeching and blistering over the loins, with partial relief. His hospital tickets (which he gave me) bore that he was considered to be suffering under "Nephritis," and that he was dismissed "Cured."

Aggravation of pain.
Treated as "nephritis."

From this period he dated his loss of appetite and of flesh, as well as his diminished strength. He attributed these symptoms to the exhaustion from continued pain, and to want of sleep. He asserted that he scarcely ever slept soundly, or for any length of time. He stated that he was of sober and temperate habits.

Emaciation.
Sleep.
Habits.

The following physical examinations were made at the dates indicated:—

May 2, 1854.—On percussing the back over the lower part of the chest, the right side is clear to the bottom, the left side perhaps very slightly dull near the spine.

Over the thoracic aorta, auscultation is normal down to a point opposite the ninth dorsal spine, or a little above, and from this downwards for two inches; here the sounds of the artery are increased

Physical Examination of the Back.
Percussion-dulness.
Increase of Arterial Sound.

Reduplication of Second Sound. Tenderness on Pressure.

and consist of two pretty acute flapping sounds (the second sharply reduplicated) in rhythm corresponding to the arterial diastole and systole. There is slight tenderness over the two floating ribs, more considerable over the two first lumbar vertebræ on the same side. No tenderness on the right side, but dull constant pain, alternated with sharp shooting pains over the whole lumbar region, and chiefly to the left.

Physical Exmination of Epigastrium. Tenderness on Pressure. Pulsation.

In front no localized tenderness excepting at the left side of the epigastrium. Here, immediately below the hypochondrium and ensiform cartilage, a slight undulating pulsation is appreciable by the eye; this becomes distinct both to the eye and hand upon pressure with a large surface, and can be traced down the aorta to the umbilicus.

Blowing Murmur.

In the epigastric and left hypochondriac region, immediately below the last rib, there is a very distinct blowing murmur accompanying the diastole of the artery; this ceases to be distinct lower down, and is not heard at all at the umbilicus.

Heart's Sounds.

Sounds of the heart loud; a reduplication between the first sound and the second, which is exceedingly well marked over the *aorta.*

Tension of Rectus Muscle (left.)

May 3d.—It is observed that the rectus muscle of the left side is stiffer than the other. The skin to-day is rather dry, but the patient is not feverish.

Characters of the Pulsation.

May 5th.—The pulsation which was felt in the left hypochondriac region, is not so distinctly felt to-day. The pulsation is more distinct from sunset till midnight.

Percussion-dulness.

Comparative dulness on percussion at level of tenth dorsal vertebra, and very distinct over floating ribs, disappearing in the lumbar region.

Lumbar Pulsation.

Throughout the lumbar region, and over the region on a level with the two last dorsal spines, the hand is raised on firm pressure, by a distinct pulsating movement.

Faint Murmur over 12th Rib.

There is a faint reflection of murmur heard near the outer end of last rib.

Thrill in Epigastrium.

In several examinations it was afterwards observed, that when the murmur was most strongly marked, a vibrating thrill could be felt by the hand in the epigastrium; which, however, was frequently inappreciable.

Treatment. Result. Dysenteric Attack.

The only treatment employed in this case was the application of numerous anodyne and stimulant liniments externally, including various proportions of opium, aconite, belladonna, chloroform, turpentine, &c.; and the use of various tonics and sedatives internally. On two or three occasions a few leeches were applied over the left lumbar region; but the results were not sufficiently encouraging to warrant frequent repetition of the detraction of blood. With the exception of temporary relief, and considerable improvement in the state of the tongue and gums, the general condition remained unchanged for the greater part of three months, when he was seized with symptoms of severe acute dysentery, under which his already exhausted frame gave way rapidly, and against which the very active administration of opiates by injection, as well as by the mouth, proved of no avail. He

Death.

died in great suffering, preserving his senses entire almost to the last.

Examination of Body.

A *post-mortem* examination, performed in great haste and under many disadvantages in consequence of objections made by a brother who was present, enabled us to ascertain the following facts:—

Thorax.

The thoracic organs were free from disease, except that the heart was very small.

Abdomen.

The abdominal organs likewise appeared to be healthy, except the colon, which was the seat of a remarkably intense dysenteric ulceration.

Aorta.

Aneurism.

Erosion of Vertebræ.

The aorta was free from dilatation, and from deposit in its coats in the thorax. Between the crura of the diaphragm it presented the upper part of an aneurismal enlargement occupying its posterior wall, and clinging to the vertebræ. The sac, which was as large as a considerable cocoa-nut, had produced deep erosion of three or four vertebræ, and of the corresponding ribs in the left side; anteriorly it displaced the left kidney, and seemed somewhat to stretch its artery and vein. The kidney was not however in any degree diseased. Below the aneurism the aorta was again normal and free from atheroma.

It is scarcely possible in this case to doubt, considering the healthy state of the coats of the aorta, that the aneurismal tumour must have had its origin in some accidental violence, the consequences of which escaped attention at the time. The single instance of injury which the patient was able to call to mind can scarcely have been the starting point of the disease. It may be, indeed, that the pains which succeeded the attack of fever were ordinary lumbago. This will appear not altogether improbable if we consider the long period of quiescence (6 years) which succeeded this first accession of lumbar pain. The idea that he passed blood in the urine during this attack is very likely to have been suggested by the views taken of his case afterwards, of the nature of which he was well aware. It is, however, possible that in the delirium or the coma of fever some injury may have been experienced, which did violence at once to the arterial coats and to the kidney, and thus gave rise to pain and hæmaturia.

The first medical diagnosis was one which might probably have been negatived by careful and repeated examination of the urine. So far as the pain, however, was concerned, the case must at this time have very completely simulated calculous nephritis. Even at the time I first saw him, I entertained some doubts on this point.

So long as the physical signs were not unequivocal, the diagnosis appeared to me to present three alternatives:—1st, *Calculous nephritis*, which would have explained the pain, but of which the urine afforded no evidence; 2d, *Organic disease of the pancreas*, of which affection deep abdominal pain, epigastric pulsation, constipation, and emaciation, are very characteristic symptoms ; and, 3d, *Aneurism of the abdomial aorta* very near the diaphragm. The first consideration was rapidly dismissed ; the two last occupied my attention for some time.

Against the presumption of pancreatic disease, there was to set the absence of jaundice throughout the affection, the absence of any marked change in the evacuations, and, finally, the seat of the epigastric pulsation, which was very high up and to the left, under the hyperchonder, while that of pancreatic disease is central, and extends towards the umbilicus. When murmur and thrill became distinctly developed, all doubt was an end. Even before this, the reduplication of the cardiac second sound, and the distinctness of the sounds in the course of the aorta posteriorly, had suggested additional probabilities in favour of aneurism.

The treatment presents little room for remark, and none for congratulation. The protracted sufferings were most painful to witness, and might possibly have been abated by a more liberal allowance of narcotics ; but nothing less than a constant use of these poisons would have sufficed for this purpose, and the patient evidently preferred to be without them, except in extremities of pain.

Case 2.

CASE 2.—A waiter in a tavern was admitted into the Royal Infirmary, Ward 5, August 1854.

Abdominal Pain, &c.

Bowels.

Appetite, &c.

Palpitation.

Chest Symptoms.

He complained of paroxysms of suffering in the abdomen, frequently recurring, and often attended with deadly faintness. The pains were described as extending from the back over every part of the abdomen, and as being of an extremely agonizing character. The bowels were not constantly either loose or costive, but had been in both conditions. He had been in the Hospital before, and had been subjected to much ineffective treatment. The appetite was not good, eating being frequently followed by pain. He also suffered at times considerably from palpitation in the cardiac and upper abdominal regions. There were no marked symptoms connected with the chest ; on strict inquiry he admitted a slight occasional cough, without expectoration. The urine and fæces were easily passed. There was no dysphagia. The intellect perfectly clear, no headache. Senses entire.

Emaciation and Pallor.

The appearance of this patient was that of one in the last stage of phthisis. He was excessively emaciated, very pale, without, however, either flushing or fever ; skin uniformly rather dry. Countenance, though much collapsed, intelligent, and not expressive of much anxiety, except during the paroxysms.

Physical Examination. Abdomen. Superficial Veins.

The chest was very thin and collapsed ; the abdomen somewhat tympanitic, moderately distended. The abdominal integuments were thin and moveable over the deep fascia ; several of the superficial veins were notably enlarged. The natural elasticity of the abdominal

X.—*Case of Sloughing of the Labia Pudendi and Vagina, followed by partial obliteration and stricture of the Vagina.*

By William Williamson, M.D., Physician to the Royal Infirmary, Aberdeen.

History. Agnes Stopani, æt. 22, native of Aberdeen, married; was admitted into the Royal Infirmary on the 13th August 1854, complaining of great pain at the private parts. Three weeks ago she was brought to bed with her third child, at the full time. The labour was in all respects natural, no instrumental interference was had recourse to. She had a bad recovery, and at the end of a fortnight the private parts began to be very painful.

She attributes her present condition to her having been neglected after her confinement, for she had no friend to render her any assistance, and the midwife never called to inquire after her welfare. For three weeks she had no change of linen, and the napkins which had been first applied remained in contact with the genitals, saturated with the discharges. She is positive that there was no case of erysipelas at that time in the neighbourhood, but states that several of the inmates of the same house were labouring under typhus fever.

State on examination. On exposing the parts, the labia pudendi were found to be in a mortified condition, coal black, and emitted a most offensive odour. While being removed from the ward to a closet hemorrhage took place to some, but not a great extent. She appeared to be so sunk and weak that no further examination was made to-day, and she was ordered to have four ounces of wine, a liberal allowance of beef tea, a grain of opium at bed time, and a poultice applied to the parts.

August 14.—Slept well, pulse 112, tongue dry, voids urine twice daily, has two stools in the 24 hours. No fæces came by the vagina. She was seen to-day by Drs Dyce and Pirie. On separating the labia very gently it was found that the phagedænic action extended deeply into the vagina, which appeared full of sloughs and pus.

She experienced such acute pain from this manipulation that it was deemed advisable not to make a further examination. Lint, soaked in turpentine linament, to be applied to the parts, and the poultices, which she dislikes very much, to be continued, as also the wine, opium, and beef tea.

It is only when the parts are handled that she complains of acute pain, and on this account the opiate was prescribed only at bed-time.

Progress of sloughing. August 21.—Both labia have sloughed, leaving a raw surface on each side, extending from the symphysis pubis to within a quarter of an inch of the anus; the perineum, with the exception of this portion, having been likewise destroyed. On cautiously introducing the finger into the vagina the os uteri could be felt free from disease.

She is gaining strength rapidly, sleeps well, without an opiate.

September 6.—The parts look healthy. A free discharge of pus takes place from the suppurating surfaces.

September 20.—A few days ago diarrhœa came on, but was restrained by mild astringents. She also suffered from an attack of acute pain in the left groin. No swelling or redness was visible, but there was great tenderness on pressure, which was subdued in three or four days by leeches and poultices.

Adhesion of vagina.

October 4.—On making an examination to-day the opposing surfaces of the vagina were found to have adhered, throughout almost their entire extent, so that it was with great difficulty the finger was introduced into that canal. The os uteri could not be reached from strong adhesion preventing its further introduction.

Attempts at dilatation.

October 5.—To-day she was brought fully under the influence of chloroform, and an attempt, which proved unsuccessful, was made, with the assistance of Dr Dyce, to dilate the vagina. Considerable force was employed, but the adhesions could not be broken down. A sponge tent was introduced into the vagina.

October 9.—The tent has proved inefficient in dilating the stricture, and, at the suggestion of Dr Matthews Duncan, who happened to visit the hospital, a silver lithotomy tube, eight and a half inches long and about the thickness of the little finger, was introduced into the vagina, and kept in situ by tapes, fastened as after an application for stone.

October 15.—Since last report the tube has been replaced several times, owing to the patient having withdrawn it, and it was only after great persuasion that she would submit to its introduction. To-day a piece of gutta percha, about five inches in length and of the thickness of the middle finger, was substituted for the tube, and retained in a similar manner.

October 21.—She did not allow the gutta percha to remain above a few hours after it was introduced on the 15th, when she loosened the tapes, and concealed it beneath the pillow. On making an examination, the finger was carried along the vagina until it encountered a small circular orifice or ring, which could not be penetrated by moderate efforts, and which did not admit the tube formerly used.

October 25.—No further attempts were made until to-day, when the introduction of the tube was effected by Dr Dyce. It passed so far into the vagina and through the stricture that a small portion of it only was visible externally. She complained a good deal at the time, but the pain soon wore off.

October 26.—Did not find the tube at all uncomfortable. About two tea spoonfuls of coagulated blood had come away. To-day the gutta percha was introduced through the circular orifice, after great resistance on the part of the patient, who was with difficulty kept in bed, and complained loudly of the pain. Considerable force was used in this operation, which was followed by trifling hæmorrhage.

Her general health is excellent. She has become stout, and her present contrasts strikingly with her former appearance. She has taken no medicine for several weeks.

October 27.—Does not suffer from the presence of the gutta percha, which was withdrawn at the visit. The circular opening is slightly enlarged. The gutta percha was again introduced after great resistance on her part, and so determined was she to leave the hospital that she demanded to have her ticket signed. This was refused, and she was persuaded to remain.

October 31.—The gutta percha was withdrawn to-day and the tube substituted. The latter was passed with great ease through the stricture, but it was found impossible to introduce the former without causing her great pain.

November 7.—The gutta percha was with some difficulty introduced four days ago, but came out yesterday. To-day the tube was substituted for it.

November 13.—The menses appeared last week, and continued to flow in great quantity for six or seven days. She could not move in bed without causing a considerable increase in the discharge. The tube was withdrawn on the appearance of the uterine discharge. The circular opening is smaller.

December 1.—For the last twelve days the tube has not been kept in the vagina, but introduced every third or fourth day. To-day a uterine sound was passed through the ring to the extent of six inches, from what may now be called the fissura magna (the length of which in a perpendicular direction is one and three-fourths of an inch). Of this measurement three and a half inches are beyond the ring or stricture, which is situated about two and one-fourth inches from the outward orifice of the vagina. It will admit a bougie somewhat larger than the tube.

December 7.—The tube has not been introduced since last report. To-day the ring was found to have become smaller, conveying to the finger the feeling of merely a slight depression. On attempting the introduction of the tube the stricture grasped it so tightly that it could not be accomplished at first, but on making a second trial and using gentle but steady pressure it passed through it with a jerk. It was retained by the tapes, and she was enjoined to keep the recumbent posture.

December 9.—The menses began to flow last night, when she withdrew the tube. Suspecting that she was practising a deception to get rid of the tube, to which she had as great an aversion as ever, an examination was made, which proved her statement to be correct.

Operation on the stricture.

December 22.—It seemed hopeless to attempt any thing further in this distressing case, but unwilling to allow her to leave the hospital without a surgical operation having been attempted, Dr Pirrie, at my request, made an incision on each side of the stricture, which caused her a good deal of pain, and was followed by a considerable discharge of blood.

She complained very much of the operation, and when forcible attempts were made to dilate the stricture with the finger, she cried out lustily, and could with difficulty be kept in bed. The stricture was of such a dense, fibrous, and, to the touch, cartilaginous nature, that it yielded very little, if any, after the operation.

December 27.—During the four days succeeding the operation she lost a considerable quantity of blood, but not to such an extent as to make any impression on the system. The hæmorrhage was restrained by plugging the vagina.

As her health has for some time past been completely restored, and nothing more can be done to remedy her unfortunate condition, she was allowed to leave the hospital with strict injunctions to apply for relief should she pass her periods without discharge.

Commentary.

One of the most interesting points connected with this case relates to the probable cause of the phagedæna. When this untoward event happens, it is almost always the result of protracted labour or instrumental interference, and occasionally of neglected venereal disease. So far as we are aware, few writers on the subject have alluded to other than these three conditions under which this accident may happen. Almost all authors speak of it in terms similar to those used by Burns and Churchill. "In consequence of very severe labour, inflammation, followed by gangrene of the vagina, may be produced."—("Burns' Midwifery," p. 67.) "If," says Churchill, "the pressure have been very great, the parts most subjected to it may mortify and slough."—("Diseases of Pregnancy and Childbed," p. 280). Erysipelatous inflammation of the vagina and labia terminating in gangrene is sometimes met with in practice—(Burns, p. 72.) Yet it must be a rare affection, for Dr Johnson of Dublin ("Hardy and M'Clintock on Puerperal Diseases," p. 45), in his extensive practice, never met with but one example of it. The case we have recorded may originally have been an instance of this, but as it did not come under observation until the disease had existed for several days, we cannot speak confidently on that point.

The most probable explanation is suggested by the patient's own narrative. According to her statement she had been neglected after her confinement. This, we think, furnishes a clue to the origin of the mortification.

After every labour a certain amount of irritation and congestion exists in the vagina. If the napkins which have been applied to the pudendum are changed daily, the discharges are removed, and are not allowed to remain in contact with the integument; but if this precaution is neglected they will soon, particularly in warm weather, become offensive, and will keep up and increase the irritation previously existing.

Two cases lately occurred in our practice which appear to strengthen the above opinion. A girl, eight years old, of a weakly frame, inmate of an asylum for children, was confined to bed by a low fever of the remittent type. At the end of three weeks the nurse, one day while changing her linen, discovered that the labia were the seat of superficial mortification. They were of a dark colour, and emitted an offensive smell, and from the vagina a muco-purulent discharge was observed to issue. Lotions, containing sulphate of zinc and nitrate of silver, with poultices, and tonic treatment, restored the parts to a healthy condition, though the patient was reduced to a state of extreme weakness. The second case was that of a child four years of age, inmate of the same institution, who was attacked with inflammation and swelling of the labia, which gave rise to great pain when obeying the calls of nature. In both cases inflammatory action was set up by a want of attention to cleanliness. This was particularly remarked in the youngest of the two children, who was afflicted, to a great degree, with incontinence of urine. The case of the elder girl presented some similarity to the description of an epidemic in 1815, given by Mr Kinder Wood in the seventh volume of the "Medical Chirurgical Transactions," but in many essential particulars it differed from that fatal affection.

Such is the view we took of the unfortunate case, the subject of this communication, when it first came under our care. At one time it was suspected that syphilis might be the cause, but careful examination failed to discover any sores of a venereal character, and the previous history did not give countenance to this idea.

With regard to the treatment pursued, great difficulties were encountered from the patient's unwillingness to submit to any restraint, and from her apparently fixed determination to oppose every effort to remedy her distressing condition. The adhesions must have been unusually intimate.

In a case recorded by Dr Lee ("Clinical Midwifery," p. 27), where the labour was tedious and the forceps were applied, inflammation and sloughing of the vagina took place, and the canal was afterwards nearly closed up by the cicatrix. "This, however," he adds, "was overcome by bougies."

In Stopani's case it was found impossible to break down the adhesion, or to effect in the least degree dilatation of the stricture of the vagina by such means.

XI.—*Case of Presentation of the Bladder in Labour.* By ALEX. HARVEY, M.D., Southampton, late Lecturer on the Practice of Medicine, in University and King's College, Aberdeen.

Mrs M——, ætat. 28, was taken in labour of her first child on the evening of Tuesday, the 4th July last, some weeks before the expected time of her delivery. She sent for me the same evening, when I found her complaining of pains in the lower part of the abdomen, both behind and in front, and likewise round the hips. She had an opiate given her, which had the effect of relieving, indeed, of removing the pains, and procuring for her a comfortable night's rest. **History.**

The pains returned next morning. On examining her, per vaginam, the os uteri was found slightly dilated. In the evening the pains had become more decided and regular, and the os uteri more fully dilated. No bag of waters could be discovered, and the patient was not aware of its having broken. The presentation was ascertained negatively. It was not the head, but its real nature remained doubtful.

At 3 A.M. of the following day, the os uteri was pretty fully dilated, and the pains were good. The presentation, however, was still uncertain. It seemed impossible to doubt that what had all the cha- **Diagnosis of presentation.**

racters of feet could be aught else, but the limbs connected with them, besides being themselves extremely small and tiny, ended in what seemed certainly not a breech, and very exactly resembled the shoulder. No anus could be felt, nor organs of generation. The limbs were brought down, and proved to be the feet and lower extremities.

The pains continued vigorous, but the child descended very slowly till about 1 o'clock in the afternoon, after which the labour advanced steadily although still very slowly, till about 3 o'clock, when the entire fœtus was expelled by one strong pain.

Altogether the labour occupied above 36 hours.

For some time before the delivery of the woman, and after the full dilatation of the os uteri, the presentation was as follows:—The front of the child was to the spine of the mother; the trochanters and head of the thigh bones, which were in connection with a bony surface of very limited extent, and (as before observed) very closely resembling the top of the shoulder, lay behind the pubis. Filling the hollow of the sacrum was the front of the abdomen, lying remarkably low down and pressing on the perinæum, occupying besides, to a great extent, the cavity of the pelvis.

At the moment of delivery, and for some time previous to this, this abdominal mass, in the form and shape of a large round swelling, which was tense, elastic, and unyielding, filled up the whole outlet; the breech, or what appeared to be the breech, being pushed upwards, behind the pubis. The mass in question passed through with extreme difficulty, and with corresponding anguish to the mother.

Description of Fœtus.

The child was still-born. On examining it, it was found that there were no buttocks, and that the bony pelvis was but imperfectly developed, small and insignificant. There was no anus nor any trace of one. A projecting fold of skin of a livid colour occupied the site of the scrotum or labia, and had a shallow imperforate fissure in the centre of it.

The abdomen (speaking comparatively) was of enormous size, and was distended by what was manifestly fluid in its interior. It encroached upwards on the chest, which was preternaturally short and narrow. The child's neck was apparently wanting, and the head, small, elongated, and livid, but not otherwise abnormal.

The general surface presented marks of incipient decomposition.

On laying open the abdomen the urinary bladder was found occupying the greater part of its cavity. It formed a round or oval tumour, considerably larger than the average size of the fœtal head. Unfortunately, it was accidentally punctured, and its contents escaping, neither the real nature nor the exact amount of the fluid it contained could be ascertained; but it had the colour and odour of urine; and, judging as well from the apparent capacity of the bladder, as from what was seen to escape, the fluid might have amounted to about a pint and a half.

The bladder had no outlet, but the ureters entered it in the usual way, and were pervious throughout. Moreover, the rectum also opened into it, but no indications of the presence of meconium were discovered in the bladder. The condition of the generative organs was not minutely inquired into, but on a cursory examination none could be seen, and the sex of the infant was not made out.

The striking feature in this case was the extreme difficulty with which the delivery was accomplished, a circumstance now manifestly referable to the unyielding nature of the fluid in the urinary bladder, which was actually the presenting part, and which, from the action of the uterus upon it, had to make its way by its broadest possible diameter.

XII.—*Lupus Exedens of the Face, fatal by Disease of the Larynx.*

We have been favoured by Dr BRODIE, Medical Officer of the City Parochial Board, Edinburgh, with the following facts, illustrative of a rare termination of Lupus.

A. C., the wife of a porter, 36 years of age, had been for many years affected with ulceration of the face, destroying the palate and laying bare the upper jaw-bone. The nose and much of the face was destroyed. The palate could be seen through a perforation in the cheek. The eyelids were eroded and raw.

Under these circumstances, symptoms of laryngitis set in. It was impossible to open the mouth, so as to ascertain the state of the glottis; but as the ulceration of the palate evidently extended very far back, Dr B. supposes that the epiglottis and laryngeal mucous membrane had either been directly involved in the cutaneous affections or in some secondary inflammation, depending on its extension. Under these circumstances, suffocation being threatened, tracheotomy was performed, but only with temporary relief; the patient survived for twelve hours, and appeared to die from exhaustion. An examination of the parts was not permitted.

At the time of the fatal result this patient had a healthy-looking and vigorous child, some months of age. A sister of the patient who survives her is affected with lupus, but much more limited in its extent.

XIII.—*Case of Congenital Goitre.* BY ALEXANDER KEILLER, M.D., Physician to the Royal Infirmary, &c., Lecturer on Midwifery and the Diseases of Women and Children in the Medical School, Surgeons' Hall, Edinburgh.

The extreme rarity of goitre occurring congenitally induces me to record the following well-marked instance, which happened in my obstetric practice a few months ago:—

On the 26th April 1854, I delivered Mrs R——, æt. 24, of her first child, which was, with the exception of a very large goitrous tumour, a remarkably fine male infant. The labour was natural, but somewhat protracted in the second stage, in consequence of the immense thyroid enlargement throwing the head slightly backwards, and thereby adding to the bulk of the presenting cranial diameter. When the head of the child, which was above the average size, passed through the outlet, the large peculiarly soft and sponge-like swelling was felt occupying the hollow of the sacrum, and as the head moved onwards under the pubes, the cushiony thyroid mass was on the recurrence of a pain slowly pressed forwards over the distended perineum, and the body of the child almost immediately followed. The tumour occupying the front of the neck was very large at birth, extending from immediately under the chin downwards towards the sternum, and projecting laterally beyond the margin of the neck, to such an extent as to be observable from behind. Whenever the child cried, the shape and character of the swelling were more correctly observed, the whole mass immediately becoming more bulging, but at the same time less cushiony, the middle as well as the lateral lobes of the preternaturally enlarged gland becoming well defined whenever the increased action of the muscles which were called into play during crying occurred. The singular appearance it presented when thus enlarged, reminded me very forcibly of the "blow" of the pouter pigeon. Though at first, from its great bulk, unseemly, it did not in any material degree impede respiration nor otherwise interfere with the health or development of the child, which did well, and still continues to thrive, it being now about ten months old, and in the enjoyment of excellent health. The swelling, although still perfectly apparent, as in an ordinary case of goitre, is very much diminished in size, and in consequence of the now increased bulk of the head and body of the child, is comparatively inoffensive to the eye unless when the child cries and throws back his head (which he does and has always done to a considerable degree during crying), when, from being collapsed, soft, and covered with loose skin, it is immediately projected forwards, and to either side, as if the individual lobes of the gland were suddenly inflated with air, which projection, however, as suddenly diminishes on the subsidence of the muscular action referred to.

I consider it proper to mention that Mrs R—— was born and brought up in Cumberland, her mother being a native of Keswick in the Lake district, where ordinary goitres are well known to be common. None of her immediate relations, however, were ever so affected, and with the exception of some slight glandular swellings in the neck, about four years ago, Mrs R—— states that she never had bronchocele, nor anything at all resembling the goitrous tumour on the neck of her child.

XIV.—*Case of Anæmic Palpitation.—Enlargement of the Thyroid Gland and Eyeballs.* By JAMES BEGBIE, M.D., President of the Royal College of Physicians.

History.

J. K., aged 32, by occupation a gentleman's servant, many years ago had a fall from horseback, by which he sustained a severe wound on the occiput, from which a profuse and continued hemorrhage took place. He has never been quite well since that occurrence. In the beginning of 1845 he suffered from bilious fever, and made a slow and imperfect recovery. In August 1851 had an attack of jaundice, which continued more or less for a whole year; and during its progress the symptoms which first attracted attention in connection with the present history developed themselves. For more than a twelvemonth he has been subject to palpitation, breathlessness, and giddiness. These symptoms were soon followed by enlargement of the thyroid gland, and by increased prominence and distension of the eye-balls, so as to give him a remarkable appearance of staring, which was noticed by all his friends.

Symptoms.

Heart and vessels.

Eye-balls.

Thyroid gland.

Pulsation and murmur.

Treatment and progress of case.

In the spring of 1853 he first came under my observation. He was a man of middle size, and well-formed. His countenance was pallid and sickly; but under excitement either mental or bodily, his face quickly flushed, and his manner became nervous and embarrassed. At all times, but especially under excitement, the action of the heart was forcible and rapid, and this action was communicated to the vessels of the neck and head. The eye-balls were enlarged and prominent, presenting the appearance of great distension. The thyroid gland was also much enlarged throughout its whole body, varying in size according to the force of the heart's action. This enlargement was accompanied by a strong pulsation over the tumour, and in the vessels of the neck; and a peculiar thrill was felt, and a loud murmur heard, over the whole extent of the gland. The action of the heart was violent and jerking; and a loud bellows murmur attended the first sound, and was heard most distinctly over the region of the aortic valves. In a state of quiet and rest these symptoms became moderated, and the patient was able to continue his domestic service, in the enjoyment of comparative health. He was directed to take persistently for months the milder preparations of iron, and to use a diet chiefly of animal food. Under this plan he improved in health, and all his more urgent symptoms subsided by degrees. In the autumn of 1853, he went to England, and continued, I understand, to improve in health; but I lost sight of him at this time. Early in the spring of 1854, he had engaged to accompany a distinguished officer to the Crimea, as his body-servant. Before the time arrived, however, when he was to have entered on his duties, he was, after exposure to cold and fatigue, seized with inflammation of the chest, and obliged to relinquish the undertaking. His illness was severe and continued, and he never recovered from it. It appears to have aggravated greatly the peculiar symptoms under which he had so long laboured, and complicated the aspect of his case. He

Complications.

Death.

was able to return to Scotland, however, and in March 1854, he again, after an interval of several months, came under my notice. At this time he had enlargement of the liver, with jaundice, and the signs of organic disease of the heart, accompanied with general dropsy. His eyes were still prominent, and the thyroid gland enlarged, but neither of these now maintained the striking character which they possessed previously to his leaving Scotland. No remedy was of any avail, and he sank on the 28th of March, worn out with the sufferings of complicated disease in the thorax and abdomen. The body was opened on the 30th by Mr Johnston, in presence of Drs W. T. Gairdner and Warburton Begbie, and myself.

External appearances.

Sectio Cadaveris—March 30, 4 P.M.—Body of a moderately stout and middle-sized man. The linens in which the body was shrouded were in many parts (as the neck, axilla, scrotum, and legs), quite soaked with serous fluid exuded from the body. The cuticle was in many parts loose and easily detached from the *cutis vera*, and in every organ of the body examined, signs of decomposition were generally met with—frequently so marked as to obscure the proper pathological conditions.

The subcutaneous tissue, and, indeed, the cellular tissue generally, contained very little fat, and was in every part more or less infiltrated with serum.

Pericardium.

On opening the thorax, the *pericardium* was found of large size, and overlapped, at its sides only, by the margins of the lungs. It contained about *six ounces* of a transparent yellow-coloured fluid. Upon the anterior surface of the heart—near to its base—a "milky spot" was observed, about the size of a florin-piece, and another upon the opposite surface of the pericardium.

Heart.

All the cavities of the *heart* were filled with dark-coloured blood in a more than usually fluid condition; one well-formed decolorized clot was found in the right ventricle. The heart was large (might have weighed 16 oz.), soft and flaccid. All its chambers, but more especially the *ventricles*, were considerably dilated; the tricuspid orifice admitted *four* fingers, the mitral *three*. The tricuspid and mitral valves were large, but otherwise normal; the sigmoid valves were also normal. The *vena cava* inferior was unusually large; and the aorta was small when compared with the size of the pulmonary artery. The endocardium and inner surface of the aorta were stained of a deep red colour.

Lungs, &c.

Both pleuræ contained turbid fluid of a dark red colour, computed at about eight or ten ounces in each. The posterior surface of the upper lobe of the *right* lung was firmly adherent to the costal pleura by strong short bands of lymph. The lower lobe of this lung was crepitant, and infiltrated with bloody serum; the posterior part of the upper lobe was condensed, non-crepitant and friable as if hepatized, but the advanced state of decomposition in which it was, prevented a decided opinion being formed concerning it. The left lung was crepitant, with the exception of its posterior part; and from the surface of a section a considerable quantity of bloody serosity was readily expressed.

Neck,

The sterno-hyoid and sterno-thyroid muscles were much thinner and broader than natural from being stretched over the thyroid body, which was of large size. The external jugular veins were normal; the internal jugulars were large—the left one when slit open measured an inch and a half across at a level with the cricoid cartilage. The thyroid body was of large size, but was not weighed; its weight may however be computed at being four or five times greater than natural. Each lateral lobe measured an inch and a half in breadth, and was of a corresponding thickness. This great increase in size was not partial but general, and although the *isthmus* was comparatively larger than the lateral lobes, there was complete symmetry of both sides. It was of a dusky-red colour, smooth, and well-defined, and slightly irregular on its anterior surface, but still retained the natural convex and semi-lunar form of the organ when in a state of health. **Thyroid gland.**

The peritoneum contained about a pint and a half, or two pints, of a clear fluid, tinged of a bright yellow. The *spleen* was enlarged in all its diameters and was computed to weigh about twenty ounces. It was of very firm consistence, and on section presented the trabeculæ well-marked, and also the Malpighian bodies, which were of an opaque yellowish white appearance. The *kidneys* were both very large and very soft. The cortical was to the medullary substance, relatively, increased in amount, and the great size of both organs seemed to arise from this circumstance; otherwise their actual pathological condition could not be ascertained on account of the advanced state of decomposition which they were in. The *liver* was certainly not enlarged, perhaps rather small; its surface was somewhat irregular, slightly and superficially fissured at points; no rounded nodules, however (as of cirrhosis), could be observed. On section the tissue was (considering the decomposed state of all the organs) rather hard and dense, and seemed partially atrophied; its colour was deep-orange, and in some places there was an approach to "nutmeg" congestion. **Spleen.** **Kidneys.** **Liver.**

This case presents a well-marked example of the disease first described by Dr Graves of Dublin, and afterwards noticed by Sir Henry Marsh, Dr Stokes, and other Irish physicians; and whose true pathological character was, I believe, first pointed out by me in a paper read to the Medico-Chirurgical Society of Edinburgh in January 1849, and subsequently published in the Monthly Journal of Medical Science. The affection has since been illustrated by Romberg and Henoch, and other German physicians, and has attracted the notice of some of our best writers on diseases of the eye. The history is interesting, as having occurred in a male, the cases on record, with few exceptions, having been seen in females. It is more particularly interesting, as affording an opportunity of examining the morbid appearances after death, the only record of which that has yet appeared being that communicated to the Pathological Society of Dublin by Sir H. Marsh, and by Basedow in Germany. In the case now related, as well as in that of a lady who had long laboured under this peculiar affection, and in whom it proved fatal also, by supervening pneumonia (the only instances which have occurred to myself of instituting post-mortem examinations), there exists a remarkable similarity in the chief morbid appearances with those described by Sir H. Marsh. These appearances chiefly consist in the very fluid state of the blood found in the heart and great vessels, in the dilatation of the cavities of the heart, and of the venous trunks, in the enlargement of the spleen and disease of the liver, and in serous effusion into the different cavities, the result of vascular obstruction.

Since the publication of the three cases related by me in 1849, I have, through the kindness of my professional brethren, seen many additional examples of this affection, a large proportion of which have gradually undergone a cure, while the remainder have benefited, or are now benefiting, by the use of iron, animal food, and fresh air. It is of great consequence to impress those suffering from this affection with the belief of its curable nature, and to urge upon them the persistent employment of the means of restoring the red particles of the impoverished blood, and improving the general health; for we have now examples before us to shew that the neglect of these rules must lead, from functional disorder of the heart, to dilatation of its cavities, and to the usual train of consequences resulting from such a morbid change.

The more extended our experience of the phenomena constituting this peculiar affection becomes, the more convinced shall we be that the point of departure from health is not in the heart itself, but in the impoverished condition of the blood, which, after a time, affects the heart and vessels functionally, and, by long continuance, involves them ultimately in fatal organic change.

XV.—*Case of Glandular Swelling, accompanied by an Exanthematous Eruption, and fatal in the third week.* By GEO. S. SUTHERLAND, Student of Medicine, Edinburgh. (Communicated by Dr W. T. GAIRDNER.)

History. On the 8th February 1855, a female child, 14 months old, was brought to the Royal Dispensary by its mother, affected with a glandular swelling of considerable size on either side of the neck, immediately behind and below the angles of the lower jaw. The swelling was hard, tender to the touch, diffused, and not elastic; there was no discoloration of the integument. The child suffered under considerable fever, was fretful, and had an anxious pained expression. The mother stated that the infant, though not yet weaned, had been stout and healthy up to the time of the attack. On 1st February it was observed to suffer from a cold in the head, with slight cough. On the 4th, it was seized with sickness, vomiting, and feverishness. On the 5th, the swelling was observed for the first time; and since this the symptoms had been on the increase.

Roseolar eruption.

On the 11th the swelling of the neck having continued to increase and the febrile oppression become greater, an exanthematous eruption appeared upon the body, presenting the characters of roseola. It consisted at first of circumscribed spots of a circular form, varying in size from that of a pea to a shilling (the larger ones being some of them annulated), but after a time these ran together into large continuous blotches, especially on parts subjected to pressure as the back of the thighs and nates, which were each occupied by a single large patch.

This eruption first appeared on the lower extremities, and subsequently the trunk and upper extremities, and lastly the face. It began to fade in each situation about the second day after its appearance at that part, and had almost disappeared over the whole body on the 19th.

It was followed by slight desquamation of the cuticle.

Up to the 18th the febrile oppression continued very great, but on this and the two following days the child appeared much livelier. The mother said that its appetite also returned, for it attempted to take food greedily.

It was chiefly nourished all along by beef tea and milk, for its mother could not give it the breast on account of its biting the nipples. The swelling of the neck continued gradually increasing up to the 20th, but still hard to the touch, and without any redness. By this time it reached a considerable way down the neck on either side in the direction of the cervical glands, on one side extending nearly half-way down to the clavicle, and towards the middle line under the chin; the tumours of either side being separated by only a narrow interspace.

The respiration was not materially interfered with, and deglutition of liquid food could still be accomplished.

The jaws were all along so firmly clenched when attempted to be opened to see the state of the tongue and throat, that this could not be satisfactorily ascertained from time to time, but they did not seem much affected.

The bowels regular up to the 19th. On 20th, an inflammatory redness appeared in front of the neck in the interspace between the swellings, immediately below the chin, and the integument felt hard and tense at the part.

Deglutition now began to be seriously interfered with, a tea-spoonful of liquid being retained for some time in the mouth before it went down.

Bowels began to be frequently open, and stools constantly passed in bed. Child otherwise appeared better and livelier.

On 21st, not much change in the symptoms in the afternoon. The difficulty of swallowing not increased, nor the respiration more difficult. The redness in front of the neck had not extended much, but the parts beneath felt doughy on pressure, as if suppuration were in progress.

Death. On 22d, at 3 A.M., the child died.

The mother having awoke in the night and lighted a candle, was alarmed at the change in the child's appearance, and the pallor of its countenance. She put it to the breast and it sucked. After this it gradually sunk as if into sleep and died. The swellings of the neck appeared to the eye and touch considerably smaller after death than they had done the day before.

Post-mortem examination refused.

Remarks by Dr W. T. Gairdner.

It is difficult to give a name to this affection. That it was a specific and contagious disease is rendered probable by the fact that a boy of three years of age, a brother of this infant, and likewise a healthy child, was seized with feverish symptoms, sickness and vomiting (but without any catarrh), on the fifth, the day after his sister; a submaxillary swelling appearing on the sixth, which increased in size up to the twelfth, and then subsided. Although all the symptoms were less intense in this case than in the other, and though there was no eruption observed (it was carefully looked for by Mr Sutherland), I think there can be little doubt of the identity of the two affections. It is likewise to be observed that the mother of these children, during the second week of her attendance upon them, became affected with slight sore throat, with enlarged tonsils, "covered," as Mr Sutherland reports, "with yellowish specks."

Were these, then, cases of anomalous scarlatina? This suspicion, which will suggest itself to every one, is scarcely borne out by the facts. No eruption existed in either child at the time they applied at the Dispensary, nor had any been observed by the mother before that period. The roseolar rash, which afterwards appeared in the first case, was at least as late as the seventh day in appearing; nor had the tongue in either child the peculiar appearance so characteristic of scarlatina anginosa. The presence of a few cases of scarlatina in the neighbourhood lends some countenance to the idea of that affection being the source of the symptoms above described, although careful inquiries failed to detect any direct source of scarlatinous infection.

From ordinary cases of mumps, these were distinguished by the position of the swelling, which was almost entirely submaxillary, instead of extending over the parotid region. Moreover, the termination of mumps otherwise than favourably is exceedingly rare; and an exanthematic eruption in this disease appears likewise to be uncommon, although M. Rilliet of Geneva mentions having observed roseola as one of its accompaniments.

XVI.—*Case of Induction of Premature Labour for Acute Chest Complication.* By ALEXANDER KEILLER, M.D., F.R.C.P., &c. Reported by HENRY MARSHALL, Resident Assistant, Royal Infirmary, Edinburgh.

The subject of this case was recently admitted into one of Dr Keiller's wards in the Royal Infirmary, suffering from a considerable affection of the lungs, for which it was subsequently found necessary to induce premature labour.

History of chest disease

Hannah Donaldson, æt. 26, was admitted into the Royal Infirmary, on the 21st of February 1855. At the time of her admission she was pregnant, and thought she was about a month from her confinement. She stated that she had been from her childhood very much troubled with shortness of breath, but unaccompanied with cough, with the exception of a short time before the birth of her first child (15 months since). Both her father and mother were asthmatical.

Two days before admission, her present attack commenced with nausea and vomiting, which were followed by a severe cough. She was seen first by a dispensary pupil, who cupped her over the sternum, but drew only a small quantity of blood.

On her admission (on the afternoon of the 21st February), respiration was very hurried and difficult, her face congested, and her pulse rapid and weak. Her cough was very severe, with frothy expectoration. On auscultation, coarse crepitation was discerned all over the posterior part of both lungs, and anteriorly moist rales were plentifully heard. Percussion was normal. She appeared labouring under a very severe attack of capillary bronchitis; and was ordered a mixture of carbonate of ammonia and decoction of senega. The dyspnœa increased in the evening, but got better as the night advanced. On the following day she was somewhat better. The difficulty of breathing less, and the pulse stronger; antimony was prescribed in small and repeated doses, the former mixture being still continued. On the evening of the next day (the 23d) she was decidedly worse, the dyspnœa appearing likely to prove fatal. The pulse became very weak. She was ordered wine, and chloroform internally, from which she experienced considerable relief. Dr Keiller saw her about 10 P.M., and determined to bring on labour. At that time the fœtal heart could be heard towards the right iliac region. Dr Keiller thought it better to deliver her in such a way as to give child the best chance of surviving; and, therefore, instead of puncturing the membranes, with the view of merely diminishing the size, and thereby relieving the chest-symptoms, adopted the following mode. Having laid the patient on her left side (though the difficulty of breathing was greatly increased

Induction of premature labour.

by this position), he introduced Dr Weir's instrument for inducing premature labour, about three inches into the os uteri, between the membranes and the anterior wall of the uterus. About a quart of warm water was injected, and a sponge-tent immediately afterwards introduced within the os. Two hours afterwards (about half past 1 A.M.), water was injected into the vagina, so as to distend it. Soon after 4 A.M., symptoms of labour first appeared, the sponge-tent being then expelled when the patient was at stool. From this time the labour advanced, the os slowly but regularly dilating, and on subsequent examination, a foot and the umbilical cord were felt presenting. The liquor amnii came away about 10 o'clock, and both feet and a large loop of the umbilical cord descended immediately into the vagina. The cord, however, was perfectly pulseless, and the case was allowed to proceed without interference, and the child was born about half-past 10 o'clock. The placenta was expelled in about half an hour. The discharges were moderate in amount, and no bad symptoms connected with the labour subsequently occurred. The chest-symptoms were decidedly relieved after delivery, and, with the exception of a few days diarrhœa, and a slight attack of pleurisy on the right side, she steadily continued to improve, and left the Infirmary on the 17th of March, at her own desire, nearly quite well.

Result.

The instrument referred to in the above Report is that invented by Dr Graham Weir, who has contrived it with the view of combining the two plans of Hamilton and Kiwisch; the former consisting of the uterine introduction of a female catheter, and the other, in the vaginal injection of tepid water; both modes of inducing premature labour acting somewhat in the same way, viz., by separating the membranes to some distance from the vicinity of the os.

Dr Weir's instrument consists of a flattened silver tube, similar to a large female catheter, and which is attached, when used, to an ordinary injecting syringe, or to a Higginson's pump, which is better adapted for the purpose, as it can be more easily worked by one hand, the other hand being thus left at liberty to properly direct the tube within the uterus.

The tube, or large flattened catheter, measures about seven and a half inches in length, and is nearly half an inch in breadth. Three inches from its extremity it is bent in a curved form, to such an extent as to throw the point of the tube about one inch off the straight line; on each side are situated five small openings, which are placed at regular intervals, extending back one and a half inches from the point of the instrument.

In using it the tube is first introduced cautiously within the os to the extent of its curve, viz., about three inches, and then, by means of the attached syringe, the tepid water is injected so as to act by slowly separating the membranes.

Rigidity and impaired mobility of abdominal walls. Liver. Spleen. Percussion-dulness. Lungs. Heart. Retraction of inter-costal spaces.

parietes was much impaired: they appeared scarcely moveable over the viscera; and the latter, though not presenting any distinct inequalities or tumours, were everywhere too dense and resisting. The liver appeared by percussion rather small, and within the hyperchonder: the spleen could not be distinctly defined, and abnormal dulness on percussion, apparently of varying amount at different examinations, was detected under the false ribs on the left side. The lungs gave no abnormal signs to physical examination, except universal feebleness of the respiratory murmur, which, however, was more feeble in the left lower back and lateral region than elsewhere. The heart beat strongly and regularly; there was considerable systolic retraction of the fifth intercostal space, and a little also of the 4th; both sounds clear all over the cardiac region.

Diagnosis.

(The diagnosis at this point was—peritoneal adhesions, possibly also pericardial adhesions; thickening or effusion about the left vault of the diaphragm in peritoneum or pleura. Lungs not greatly diseased. Tubercular peritonitis ?)

Blood.

The state of the spleen being uncertain, the blood was examined microscopically. No excess of white corpuscles.

Treatment.

The treatment was chiefly by local applications and enemata, with regulation of the bowels.

Progress of case. Blowing murmur in epigastrium. Pulsation, &c., in epigastrium.

A few days after admission, several paroxysms of pain having occurred without material change in the general or local conditions observed, his complaint of palpitation attracted more special attention to the precordial region and adjacent parts. A blowing murmur was then discovered, having its maximum intensity in the left half of the epigastric region, but audible also in the right epigastric and upper part of umbilical regions, as also over the false ribs anteriorly on the left side, and as high as the apex of the heart. A distinct pulsation, and at times a slight vibratory thrill could be detected at the point of maximum murmur. Both pulsation and murmur were distinctly post-systolic, as compared with the heart's impulse, and appeared to correspond closely with the diastole of the abdominal aorta. No change took place in this murmur up to the patient's death. No murmur or other alteration could be detected along the spine.

Percussion-dulness.

One or two days before death, the dulness on percussion over the left false ribs was observed to be rather greater than before, but very ill-defined in character.

Mucous expectoration. Death.

Death occurred rather suddenly a fortnight after admission. For some days before, he had been expectorating a little muco-purulent sputum for the first time.

The body was examined in my absence by Mr Falconer, the resident physician, who was unavoidably much hurried in the performance of this duty. He gives me the following notes.

Examination of body.

Surface of body pale; much emaciation.

Chest.—Pericardium normal; heart normal in size, pale in colour; Lining membrane of aorta of a pink colour; lungs normal, the left having adhesions to the diaphragm.

Abdomen.—Liver normal, left lobe less than usual; spleen soft and friable; kidneys normal.

Peritoneal adhesions. Extravasation of blood. Left kidney. Aneurism. Erosion of vertebræ.

There were extensive peritoneal adhesions of the intestines, both to the anterior abdominal wall and to each other. Three pints of fluid and clotted blood were found in the abdomen among the adhesions, and forming a large bloody tumour in the left hypochondriac and iliac regions. The left kidney lay imbedded in this tumour, which was found to communicate with a large aneurismal sac, involving the aorta from the origin of the superior mesenteric artery to three inches below the renal arteries, the vessel being dilated on both sides, but chiefly towards the vertebræ, which were greatly eroded opposite the aneurism.

The arteries which arose from the dilated portion of the aorta were quite pervious.

This case presented several diagnostic difficulties; indeed, in the absence of distinct physical signs, it is more than probable that its nature would have been entirely misapprehended. I have indicated above the direction in which my own views of it tended, before the recognition of the epigastric murmur and pulsation.

The violent dragging abdominal pain, the peculiar rigidity of the parietes, the percussion-dulness in the left flank, the enlargement of the superficial veins of the abdominal wall, seemed, when connected with the emaciated and anemic habit, to form the picture of a tubercular or cancerous disease of the peritoneum, accompanied, perhaps, by mesenteric or other tumour. The complete integrity of the lungs (so far as physical signs were concerned) will not be considered at variance with the idea of tubercle, by those who are aware how frequently the affection described by Dr Baron is found to be unconnected with tubercular disease of the lung. On the other hand, a cancerous affection might readily have caused all the symptoms and signs in question; and these solutions were so much the more probable, that two circumstances, rarely observed in connection with aneurism, concurred in this case to distract the attention. These were, the peritoneal adhesions, and the enlarged state of the superficial veins. Dr Stokes, whose masterly analysis of the symptoms and signs of abdominal aneurism is one of the most important and instructive of his contributions to diagnosis, observes that he has never met with these conditions in aneurism of the abdomen; and remarks, that their frequent occurrence in other organic diseases of this cavity furnishes valuable aid in distinguishing the former from the latter.[1] I do not doubt that this observation holds good in general; but it is the more necessary to point out exceptional cases.

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[1] Diseases of the Heart and Aorta, p. 642.

The extraordinary degree of emaciation which was present in both of these cases, and which so frequently attends on abdominal aneurism, has scarcely received a satisfactory explanation. Physical suffering of the most extreme and protracted kind is sometimes endured in neuralgic and other cases, without material loss of weight; and in thoracic aneurism, I have seen the sternum, the ribs, and the vertebræ, extensively destroyed by very large tumours, without any approach to the exhaustion of these cases. Morgagni has related two cases,[1] one recorded by Valsalva, the other by Santorini, in which aortic aneurisms produced pressure on the thoracic duct, and distension of the lacteal vessels throughout the mesentery. The latter appearance was not observed in either of the present cases. Possibly the pressure on the nerves of the intestinal canal, or on the pancreas, which lay immediately in front of the sac, may have interfered with the digestive process, and thus led to emaciation. The subject, however, is one deserving of careful investigation.

I would remark, finally, that the treatment of internal aneurisms by the joint influence of starvation and bleeding (commonly called Valsalva's method) derives little support from the consideration of the natural course of these affections, in such cases as the present. In a very considerable proportion of aneurisms when situated in the upper portion of the abdominal aorta, the disease is followed by such a degree of emaciation and exhaustion as leaves nothing to be desired by the most heroic practitioner. Yet I do not remember to have heard or read of, and certainly I never saw, an aneurism of this part of the vessel which presented any considerable traces of such reparative process as is of frequent occurrence in aneurisms of the arch. It has appeared to me, on the contrary, that under no circumstances do aneurismal tumours display a more unequivocal tendency to rapid diffusion and attenuation of the sac, than when threatening to produce death by emaciation and exhaustion. In one case recorded by Dr Bennett,[2] in which the lancet was liberally used, and the other elements of Valsalva's treatment were partially carried out, an aneurism of the superior mesenteric artery was observed to diminish remarkably in size, and to become consolidated; but there can be little doubt that this was owing to the influence of another aneurism of very large size higher up in the vessel, which had been overlooked during life, and which, by its rapidly progressive expansion, had greatly diminished the impulse communicated to the lower tumour. The case, therefore, bears rather against than in favour of a system of treatment, which I must confess I am little disposed to imitate.

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[1] De Sedibus et Causis Morborum. Epist., xvii. 14, 15.

[2] Monthly Journal, vol. xv., p. 69.

VII.—*Case of Labour complicated with Hydrocephalus of the Child.* By MATTHEWS DUNCAN, M.D., Lecturer on Midwifery, &c. &c.

Mrs H., æt. 33, had had a living healthy child 13 years previously. She was married in March 1853, and became pregnant in September of the same year. After the first quarter of pregnancy was past, she was every four weeks taken with most severe sickness and retchings, the latter preventing her retaining the smallest quantity of food. These attacks lasted for a few days only or for a week. She at the same time suffered much pain in the lower part of the abdomen, and once, about the middle of pregnancy, she had a flooding, which was arrested by simple means. The greatest, and almost the only relief to the sickness was procured in the latter attacks by the use of belladonna externally, in form of a plaster of ointment, and internally, as a pill in small doses.

Symptoms during pregnancy.

Antidote to sickness.

Labour came on early in the morning of June 11th, and before noon there came away per vaginam, a very great quantity of liquor amnii. I was immediately sent for, but did not arrive till about 1·30 P. M. At this time she was sitting up, the pains were good and regular, but did not cause any considerable suffering. The os uteri was dilated to the size of about a crown piece. It was moderately soft and dilatable, and even now appeared to be slow in expansion, from the absence of the natural pressure on, and through it. The presenting part projected a little through the circle of the os, and had an extraordinary and unmistakeable semi-solid feeling, becoming tenser and harder during the pains. The nearest illustration I can use, is to say that it felt like a bag of lard. No bone whatever could be felt in the part bounded by the circle of the os uteri, but on urging the finger higher up between the uterus and the presentation, a serrated edge of preternaturally moveable bone could be here and there reached by the finger. The child was alive, and the fœtal heart's pulsations were about 130 in the minute, and heard between the umbilicus and the pubes. From these circumstances, I concluded that the presenting pact of the child was one or other extremity of the part corresponding to the sagittal suture in a head distended with fluid.

Symptoms during labour.

As there were no urgent bad symptoms, the labour was allowed to proceed, as it did, slowly, for several hours. During this time, and till the end of labour, the severity of the pains was annulled by chloroform-inhalation. She was not, however, altogether insensible at any time; and, on being asked, described her chief pain as being a feeling of bursting or severe contusion across the body, in the region of the pubes,—a symptom which is characteristic of such cases.

A symptom during labour often characteristic.

During the afternoon, the dilatation of the os uteri proceeded very slowly. The urine was retained, and on being drawn off, was examined and found natural. In the evening, considerably more uneasiness across the region of the pelvis was complained of. The os uteri was nearly but not altogether fully dilated, but this, only from want of the natural pressure upon it, as the subsequent history of the labour showed.

Kind of operative interference.

About 10 P. M. I punctured the head at the presenting part with a long and slender trocar and canula, and drew off a large quantity of serum, 8 ounces of which I collected in a clean glass jar for examination, which was subsequently done with much care by Dr George Wilson. The presentation immediately collapsed, and soon the serum only flowed during a pain. The head now began to make progress, and the trocar was withdrawn. It rapidly descended, and on passing over the perineum, the minute wound in the scalp again gave issue to a jet of serum which continued during the pains. The fœtus was born about half-past ten, and delivery was soon after completed.

Successful result.

Description of child.

The child was full-sized, but somewhat emaciated. It was alive, cried with moderate strength, and lived for two days, during which time the horrid aspect of the child prevented the attendants from taking care to nourish it. Its large collapsed head measured now 16 inches in the occipito-frontal circumference. It had harelip and cleft palate. There was an elongated excoriation above left zyoma, extending to over the parietal bone; it was connected by a line of excoriation with the left eyelids which were drawn upwards and outwards. Left nostril closed. Small skinny projection below internal angle of left eye. The testicles had not descended. On the right hand, the four fingers were irregularly united together, some by complete, others by incomplete webs. Forefinger had a constriction around it over second phalanx. The three last fingers had only a trace of the position of the nail. The three middle fingers of the left hand were united and drawn together at the points and curved forwards. No trace of nails on them. The forefinger had a thready tag attached to its point. The three middle toes of the right foot were united and nailless. Attached to the point of the fourth toe was a thready tag, enclosing a nail. The left was in the state of varus, and had a deep constriction on the first phalanx of second toe. A little more than a half of the placenta was healthy. The rest was dense, tough, elastic, much thinned, and of a rich yellow colour. It presented none of the characters of fatty degeneration under the microscope.

The mother made a good recovery.

VIII.—*Case of Amputation of the Foot without removal of the Astragalus.* By JOHN TRAILL, Esq. Surgeon to the Arbroath Infirmary, and Member of the Medico-Chirurgical Society of Edinburgh.

Mrs Reid, æt. 53, a short, healthy woman, rather inclined to corpulency, admitted into the Arbroath Infirmary Feb. 2, with disease of the right foot.

History. Accident by machinery.

She states, that when about ten years of age she met with a

Ulceration.

Contracted cicatrices.

Abscess.

severe accident from machinery, by which the inside of the thigh, about midway between the knee and hip-joints, was deeply lacerated. This wound gave rise to profuse hemorrhage, and the limb was saved with great difficulty after a residence of many months in the Dundee Infirmary. She regained the full use of the limb, but with impaired sensation of the whole parts below the wound. About seventeen years ago, during pregnancy, she says that blisters began to form about the roots of the toes, which seem to have been followed by ulceration. These ulcers healed with difficulty, and on their cicatrization the toes were found adherent, and so contracted that their points are now firmly attached to the sole of the foot, and the whole covered by a continuous cicatrix, the only marks of the digital interspaces being on the dorsal surface. About five years ago she began to have severe pain in the heel, which after a time terminated in abscess, and discharged a large quantity of matter. This opening closed after two years, but soon re-opened, and has continued so ever since.

Present state.

Caries of os calcis.

There is now a large and deep sloughing ulcer in the centre of the under surface of the heel, at the bottom of which the *calcaneum* is felt exposed and carious; below the inner ankle are two fistulous openings communicating with that under the heel, and through which also carious bone is felt. The motion of the ankle joint is quite unimpaired, and the caries appear to be confined to the *os calcis*. The whole foot below the ankle is devoid of sensation, but from the ankle upwards sensation is but slightly impaired.

The author's views of the case previous to operation.

I presume the laceration of the thigh must have involved the femoral vessels and nerves, and that the consequent impaired action of these has been the cause of the diseased state of the foot.

Had the anterior part of the foot been in a healthy state, I should have felt disposed to have removed the *os calcis* alone, but the deficient nervous supply to these parts led me to fear that sloughing might follow such an operation.

As there was evidently no scrofulous taint in the constitution, and therefore no reason to dread a recurrence of carious disease in the unaffected bones, I thought this a favourable opportunity for amputation at the ankle without removal of the astragalus,—an operation of which I had lately observed reports of two cases by Mr F. Simon, and which it seemed to me might, under favourable circumstances, be preferable to the operation of Professor Syme.

Operation.

Feb. 4.—I this day performed the operation. From the great loss of substance by ulceration in the centre of the heel, I was obliged to form two lateral flaps, one of which was larger than the other, so as to remove the cicatrix from the central line of the stump. I found no difficulty in effecting disarticulation of the *os calcis* and *naviculare;* and when the flaps were brought together a very neat stump was formed, its cavity being well filled up by the astragalus.

Its result.

As expected, the disease was found to be confined to the *calcaneum;* but this was almost entirely hollowed out, so as to form a mere shell. There was some sloughing at the posterior part, where the integuments had been weakened by long-continued ulceration and undermined by sinuses, but a considerable part in the centre united at once, and the woman made a rapid and favourable recovery. She left the house

Feb. 18, six weeks after the operation, with the stump quite cicatrized. A fortnight after, when the cast was taken, I found the stump quite firm, and able to bear the whole weight of the body, so that she was able to go about her usual household occupations.

Since the above was written, secondary amputation below the knee has been performed, owing to the formation of two extensive irritable ulcers above the ankle,—a consequence no doubt of the defective nervous supply of the limb. The cicatrix of the previous amputation was *perfectly sound*, and *firmly adherent* to the bone, the *astragalus* itself being quite healthy. The circumstance of secondary amputation having been required does not, in my opinion, in any degree militate against the propriety of this operation.

IX.—*Case of Croup following Measles; tracheotomy on the 9th day; obliteration of the orifice of the Glottis above the canula; death on the 46th day after the operation.* By Dr C. Weber of Darmstadt. (Abstract.)

A boy of three years and a half, in good health, but having a very arched chest, was seized on the 8th July with measles. He had at first no medical attendance, and no remarkable symptom until the 18th July, with the exception of a slightly aphthous condition of the mouth. Between the 18th and the 23d the child became affected with croupous dyspnœa, which increased so much as to threaten suffocation at the latter date. Dr Weber was then called in, and after employing a number of the ordinary remedies, was obliged to perform tracheotomy on the morning of the 27th, when asphyxia appeared to be impending. Improvement did not begin till an hour after the operation. The two succeeding days, though the pulse continued frequent (130–160) the respiration was quite free, except when the tube was obstructed; considerable quantities of pus were evacuated by the canula. On the 4th August, improvement exceeded all expectation; the child slept; respiration was quite free; there was no fever; the child played and ate as usual. On the 5th a trial was made to remove the canula (a previous attempt had been made without success); the moment it was withdrawn a violent attack of suffocation took place with involuntary defecation and cyanosis of the face. At the sametime a membranous mass of the size of a florin, of considerable consistence, and having an almost gangrenous odour, was expelled by the wound; but the dyspnœa continued, and required the restoration of the canula. It was thus evident that the rima glottidis was contracted, and Dr W. was led to fear that the contraction would be permanent.

Measles.

Aphthae.

Croup.

Tracheotomy.

Results of operation—Improvement.

Mass of gangrenous matter ejected by wound—continued improvement.

Between the 5th, and the 11th September, Dr Weber, becoming convinced that an organic obstruction existed at the glottis, endeavoured on various occasions when the canula was removed, to open a passage upwards, by the introduction of various sounds by the wound.

Organic obstruction of glottis.

Sounds of silver or whalebone, whatever their form, were arrested about an inch above the external fistulous opening, and threatenings of suffocation prevented these attempts from being pursued. Attempts to pass the obstacle by the mouth were equally unsuccessful. After consultation with some surgical friends, Dr Weber now proposed a second operation in the longitudinal axis of the larynx, and had already determined upon the method of operation when the sudden death of the child put an end to his plans. On the 11th September, at 6 P.M., the child was very merry and was playing in the courtyard; at 8 P.M. he had supper, and half an hour afterwards, when he was undressed, in order to be put to bed, there occurred some convulsive movements, first in the face, then in the hands, afterwards generally. A quarter of an hour afterwards, he was dead, without having presented either true dyspnœa or cyanosis.

Convulsions and sudden death.

Examination of larynx.

The relations would only permit examination of the larynx. This was made with great care by Professor Henle of Heidelberg.

The mucous membrane of the epiglottis, and of the superior and inferior vocal cords was smooth, only a little swollen, and presenting slight enlargement of the glandular openings.

Immediately below the inferior vocal cords, the larynx was sealed up by an imperfectly fibrous, firm, non-vascular substance, in which the mucous membrane was lost. The fistulous opening was everywhere lined by a pseudo-membrane, which presented a pavement epithelium; at the laryngeal extremity, this membrane ended in a pad surrounding the internal wound, and composed of a substance like that of granulations; a portion of this pad seemed as if torn, and Dr W. considers it possible that some of it may have become detached and impacted in the air passages, causing the sudden death.

Possible cause of death.

[We have abridged this very interesting and important case, which the reader may find recorded at length in Henle's Zeitschrift für Rationelle Medicin, 1853, t. 3; or in L'Union Medicale, 1854, t. 8, No. 88. We omit altogether a very minute, but not very satisfactory account of the changes in the cartilaginous parts, the only practical result of which is an impression on the mind of Dr W., that *disease* was not exclusively responsible for the occlusion of the larynx, which was due in part to a *congenital malformation* of the cartilages. We confess we should have thought that any congenital malformation tending to the result described would have produced distinct enough changes during life. We incline to attach more importance to an admission of Dr W., that the cricoid cartilage was probably cut through in the operation. Both cricoid and thyroid were much altered, and the arytænoid cartilages were displaced. The moral of the whole, is, we think, the importance of preserving the cricoid cartilage intact in this operation.—EDITOR.]

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